A. Jamme

# Miscellanées
# d'ancient arabe
# XVIII

This 1995 revision
annuls the 1994 text

Washington
July 6, 1995

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# Foreword

After several years of silence due to uncontrollable circumstances, the publication of my Miscellanées d'ancient arabe, the last number of which is vol. XVII in 1989, is now resumed in 1994 with several inevitable arrears.

This XVIIIth volume contains ten contributions dealing with both North- and South-Arabian and illustrates once more the power of pan-classical Arabism, crooked dabbling and personal blandishments in scientific research although scientific research is by definition, at least as far as I know, a studious examination prepared strictly according to the principles of objectivity in perfect disinterestedness and absolute accuracy.

This volume contains the ten following inquiries, viz

I - The A.J. Drewes - R. Schneider tandem's mistreatment of Sabaean marks found in Ethiopia.....p. 3

II - The inscribed Luristan (?) bronze sword: Danish national museum 14646 a and b, and Ja 3232..... 54

III - The inscribed Qatabanian antiquities of the Yale Babylonian Collection (YBC)..... 71

IV - The 1988 revised edition of A.H. al-Scheiba's 1982 thesis (rewritten in June 1995)..... 81

V - Turner 1: Jacques Ryckmans' homage to O. Löfgren..... 90

VI - Fâwite 1: ᶜÂgil's tombal inscription from Qaryat al-Fâw..... 118

VII - Thamudic QalḥatT 1, north of ᶜAqaba..... 155

VIII - W.W. Müller and Ja 669..... 158

IX - The calligram Ja 3282 = RÉS 2690..... 164

X - C. Robin and Ja 3230, 3231 and Mleiḥa 2 (rewritten in June-Juillet 1995)..... pp. 169-187

It would be strictly academic to determine which of the ten preceding sections is the most valuable. Each of them illustrates in its own particular way that personal interests prevail at the expense of straight research and are complacently endorsed not only by popularity-seeking individuals but alo by the sponsors of those writers, who prefer to look the other way rather than to make sure that the scientific reputation of their society remains unscathed; cf. the disheartening example of section I. Finally, for the benefit of the reader, the ten following investigations are introduced by the list of both antiquities and inscriptions either published for the first time or provided with new interpretations.

**New antiquities and new interpretations.**

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Thamudic

HaLi 434 a and b..... 157
QalhatT 1..... 155

The paginal numbers refer to the reading of the texts or the description of the antiquities even when the whole presentation of the case takes more space.



## I - The A.J. Drewes-R. Schneider tandem's mistreatment of the Sabaean marks found in Ethiopia.

Nearing the end of 1992 and before turning the inaugural page of 1993, I wish to write some notes on the Drewes-Schneider tandem's work of RIE, I and II. I had hoped that the 3rd volume of this Recueil... would have appeared during the last few months. The illogical parting of an introduction and the reading of the texts from their commentary will necessitate the duplication of numerous subtitles and subdivisions as well as the addition of uncountable cross-references at the expenses of the user who will have not only to pay for such a prodigal extravaganza but also to purchase an extra king-size desk allowing the simultaneous display of the three volumes. Furthermore, without vol. III no study of RIE can be complete. Sadly enough, however, vol. I and II provide the reader with enough material to supply a whole volume of corrections and remarks.

The following introduction and the 1st section, B, which contains three short identity cards, viz a calligram (Ja 3261), a double name (Ja 3262) and a single name (Ja 3263), were written in December 1992. The last two sections, C and D, present, respectively, open-worked metal marks (Ja 3264-3278) and three small seals of stone (Ja 3279-3281). They were composed in early 1994 after the thorough revision of the first part when it became apparent that the printing of vol. III of RIE was not planned to materialize in 1994, as De Boccard's Catalogue général 1994, p. 39, does not mention the volume.

### A - Introduction.

1 - The most intriguing statement of the "Avant-propos" is that "le Professeur André Caquot [] a encouragé et soutenu le projet" (p. 14), thus securing the necessary backing for the safe journey of the work to and through the Academy. His interest further instigated him to have "accepté de relire les épreuves" (cf. l.c.). The two coauthors duly express their gratitude to their benefactor and also to "le Professeur Jean Leclant, Secrétaire Perpétuel de l'Académie des Inscriptions et Belles-Lettres, qui a bien voulu admettre l'ouvrage dans la collection de cette Académie" (cf. l.c.). No patronage, however, can be an umbrella or lightning-rod shielding the protégé against the scalds or thunderbolts of criticism. F.i., the reader finds three variant spellings of one proper name, viz

"Méroé" (p. 8);
"Meroè" (pp. 359, 361, 385 and 387); and
"Meroe" (p. 516)!

The 1st and 3rd spellings probably are simple misprints. Unfortunately, they are not exceptions; cf., e.g.,

p. 27, l. 30, "constituzione" and "une" to be corrected to "costituzione" and "una;" and add to l. 32 the reference to "BSGI, l.c., p. 120;"
p. 28, l. 19, correct "septembre" to "septembre;" and
p. 288, l. 11, correct "à écrit" to "a écrit."

The reader is entitled to conclude that the proofreader and even an extra reviewer apparently left the work unfinished. More serious, however, are the remarks made below, which will remain valid whatever the commentary of vol. III may claim or pretend to be. Vol. I contains a text characterized by superficiality and an amateurish knowledge of South-Arabian matters and is strewn with too many inaccurate data of information. Vol. II shows the Drewes-Schneider tandem's photographs to be too grayish as in another publication of the same Academy, viz C. Robin's Inventaire des inscriptions sudarabiques, I, B (Paris, 1992), viz the great majority of those photographs are either very difficult to read or even useless. It is, then, like entering into an oasis of light and freshness to find pl. 165 that, although still grayish, is clear enough to be very useful.

2 - Also, as in the same C. Robin's publication (with the exception of pl. 48, top), the wide-spread omission of all scale in the photographic documentation too often deprives the reader from an essential documentation on all items, viz out of 345 photographs, only eleven have some kind of scale, viz

a wooden cm zigzag rule in RIE 11, 71, 72 (top);
a tiny section of an unidentified scale in Ja 3279; and
a handmade square cm bar in RIE 33 (twice), 219 (thrice) and pl. 185 (twice; two unnumbered items!!!);

thus, one for every 31.36 photographs has a scale!

3 - In the tandem's bibliography of the publications used below, the following corrections should be made, viz

in AnAnM (p. 530, 3rd entry), correct "142" to "86, 93-144;"
in AnCFY (p. 529, 9th entry), correct "232" to "236;"
in J.Th. Bent (p. 530, 6th entry), correct "1893" to "1896;"
in CoFH (p. 531, 1st entry), correct "86" to "87;"
in DrScDEE (p. 532, 11th entry), correct "102" to "106;"
in F. Hommel (p. 533, 18th entry), correct "Südarabische" to "Süd-arabische;"
in LeMAA (p. 534, 12th entry), correct "Musee" and "Addis Abeba" respectively to "musee" and "Addis-Ababa" which can be found in almost every single page of the paper;



in D.H. Müller (p. 535, 7th entry), the 2nd half of the title is omitted and reads as follows: "nach Abklatschen von J. Theodore Bent, Esq.;"
in H. Salt (p. 537, 2nd entry), erase erroneous "Londres, 1814 etc;"
in H. Schneider (p. 537, 6th entry), correct "62" to "66;" and
7th entry), correct "92" to "96."

4 - There is a complete lack of cross-references in the study of each item to the different introductions.

5 - An obvious illogicality comes from the introduction positing that "les difficultés seront discutées dans un commentaire" which "le tome III contiendra" (p. 12). But, vol. I is already described as having a "commentaire [] réduit au minimum" (p. 11). RIE's whole presentation of the case is nebulous; it is obvious that the two coauthors did not know how to determine the accurate content of each volume. At any rate, the tandem's study of Ja 3263 (cf. below) contains, beside an introduction with the reading of the text, a "Commentaire" of 5 lines explaining the coauthors' difficulties. This example remains unique in all the other items studied below, whether the exposé of the tandem's difficulties takes 2, 3 or 4 lines; e.g., in Ja 3269, 3273 and 3264, respectively. In the balance of the volume, however, the tandem offers many other examples of his special "Commentaire," e.g., of 13 and 15 lines in RIE 343 and 345, respectively. But, many other presentations of similar difficulties are not introduced by the word "Commentaire;" e.g., the 9, 10 and 14 lines of RIE 394 (p. 485), 403 (p. 489) and 388 (pp. 481-482), respectively. "Devine qui pourra" where the two coauthors draw the line ... providing there is one... of course!

6 - From the buyer's viewpoint, the most characteristic of RIE, I and II, is the systematic waste of paper; the text is so diffuse and many of the facsimiles and photographs are so clearly outsize that the reader quickly gets the clear impression that the main goal of the printer was to multiply the pages. A good example of vol. I is the 15 consecutive pages 388-402 that may be presented as follows:

5 blank pages: pp. 388, 390, 394, 396, 402;
1 title with 4 lines: p. 389;
1 title of 1 line + a stopgap of 7 misplaced lines: p. 391;
1 title of 3 lines + a stopgap of 5 superfluous lines: p. 395;
1 title of 1 line + a stopgap of 5 misplaced lines: p. 397;
1 title of 2 lines + a stopgap of 6 superfluous lines: p. 401;
2 pages with 52 lines: pp. 392-393; and
3 pages with 75 lines and a small facsimile: pp. 398-400.

It should be added, however, that the five titles are unnecessary repeats of sections of the "Table des matières" often followed by the names of the two coauthors ... as if the reader was a toddler in need of being repeated the very same message over and

over again! This kind of outrageous repeat sometimes oversteps the bounds; e.g., the upper title of p. 8 is repeated five times in pp. 215, 237, 289, 359 and 389!

In vol. II, many photographs are uselessly outsize; e.g., RIE 4, 6, 11, 12, 13, etc; a single photo occupies the whole page; e.g. pl. 5, 10, 11, 12, 17, etc; a perfectly useless photograph on pl. 89, bottom, RIE 180, one of the archaeological jewels excavated in Ethiopia;

2 examples of prodigality are pl. 153 and 162, where a 2.5 cm wide horizontal strip would have sufficed instead the 8.5 cm, and a 1.5 cm strip instead of the 7.7 cm, respectively.

7 - The JE siglum also is given a special treatment.

a - In the course of vol. I, the JE part of the siglum is always printed in italic, but never in the concordances (pp. 517-520 and 526B-527B). I failed to find even as much as the mention, much less the justification, of this double standard.

b - Another double standard concerns the mention of the JE siglum in the identification of the texts, viz the omission of the siglum occurs quite often; cf., e.g., RIE 7, 15, 16, 19, 37, 38, 39, 47, 55, 56, 57, 58, 59, 60, 61, 62, 63, 65, 67, etc.

c - In two cases only, the number of the siglum is introduced by a zero; viz "062" in RIE 25 (p. 99) and "063" in RIE 20 (p. 94) must be corrected to "62" and "63," respectively, p. 526B.

8 - It should also be emphasized how symptomatic of some basic disorder is that the two coauthors would together and repeatedly fall into the very same errors. One would at first sight have thought them to help each other to perform reasonably. Regardless of how they distributed or organized the work among themselves,- the two coauthors state that their contribution "a été préparée ... en collaboration" (p. 12), an information which does not explain anything, - both are thus accountable for every statement printed in their joint sections of vol. I. That their myopia is voluntary and discretionary is evident in the epigraphical facts which they apparently eliminate or acknowledge at random; e.g. their very first choice of reading Ja 3261 eliminates two aslant lines that are retained in their two other readings!!! It is, thus, undeniable that for the Drewes-Schneider tandem objectivity as such is nonexistent, but rather is at the complete mercy of their joint fanciness and/or preconceived idea, whatever the case may be. The question, however, remains: how is it possible for either one to act so irresponsibly after spending so many years in the field? It is, alas!, quite possible when the yardstick of truth is the individual's own subjective whims or preconceptions, instead of the written word. In short, the Drewes-Schneider tandem's voluntary myopia is another form of the total subjectivism displayed by many authors, such as A.F.L. Beeston and his followers, whose policy is to brutally impose to the texts their own preconceived interpretations. The present case



further illustrates the Fulani proverb: "Ce qui n'a pas de tête ne va pas; ce qui a deux têtes va moins bien encore."

B - Three short identity cards engraved on stone or shard.

1 - Ja 3261 = RÉS 2630 = RIE 43 (p. 122).

Here, as in many other instances (cf. above, p. 6), the reader is forced to go to the "Concordance des inscriptions" at the end of the volume (pp. 517B and 528A) to find out that this calligram is RÉS 2630 ( for other similar cases, cf. below, Ja 3278-3281). Instead of this necessary information not listed in the bibliography of the item, the reader is referred to the profitless "F. Hommel, Chrestomathie (1893), p. 66," who limits himself to a copy of the calligram followed by "p. 432" of H. Salt, A Voyage...

The first item of the bibliography is "H. Salt, Voyage en Abyssinie, II, 210" (cf. also p. 537, 2nd entry). Again, it is strange for a work published by the French Academy to refer exclusively to the French translation of a foreign book, as if there was no copy of the original in the "Bibliothèque Nationale;" and this fact alone could only be a lame excuse. But, there is one copy there; cf. Catalogue général des livres de la Bibliothèque Nationale, Paris, 1961, vol. 161, col. 1019. This entry also reminds the reader that the Drewes-Schneider tandem's information on "Londres, 1814" (cf. already above, p. 5), which they sheepishly copied from RÉS, V, p. IV, must be deleted as erroneous, as the Catalogue... mentions "Paris, Magimel, 1816, 2 vol.;" and this information is clearly printed in p. III of both vol. I and II. The Drewes-Schneider tandem also follows in RÉS's footsteps when passing over in silence the name of the translator, P.-F. Henry, also plainly printed in the same p. III of both volumes. The preceding details on H. Salt's book and its French translation are fortunately given by E. Bernand in p. 363, where the initials of Henry, "P. F.," must be corrected to "P.-F.," and "to the Interior of that Country (1809-1810)" must also be corrected to "into the Interior of that Country ... in the years 1809 and 1810." The calligram of Ja 3261 is found in H. Salt, A Voyage..., p. 432 (cf. already above), where it is printed upside down as well as in Henry's translation (II, p. 210), a detail which Drewes-Schneider fails to mention. The same tandem says that "Sa [= of the stone] localisation actuelle est inconnue," but fails to give the only information known so far, viz that the stone was seen "among the Ruins of the Monastery of Abba Asfé" (cf. H. Salt, l.c., p. 431). Finally, it should be noted that the names of H. Salt and P.-F. Henry did not find their way in C. Robin's Corpus des inscrip-

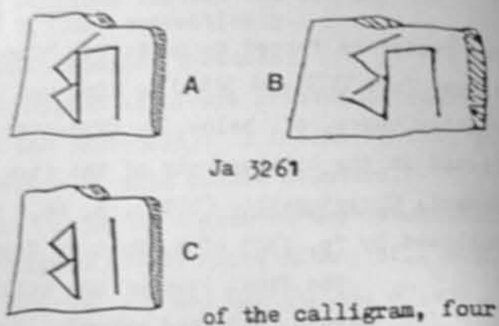
tions et antiquités sud-arabes. Bibliographie générale systématique (Académie des Inscriptions et Belles-Lettres), Paris, 1977, pp. 417A and 427A. For the difference between "sud-arabe" and "sudarabique," cf. already my discussion in JaMAR, XIV (1985), pp. 63-64; and RIE utilizes also "sudarabique" (pp. 24-26, 31, 37-39, 41-44, 46) as well as the novel "sud-arabique" (p. 28); for other cases, cf. above, p. 4.

Here follow the three facsimiles of the stone discussed below, viz

A: published upside down by H. Salt, l.c., p. 432;

B: published upside down by P.-F. Henry's French translation of H. Salt's English volume, II, p. 210; and

C: the only purpose and value of this design is to show what of the original A is retained by the Drewes-Schneider tandem's first reading which is their first choice of three readings!




Ja 3261

In the Drewes-Schneider tandem's description of the calligram, four errors must be corrected, viz

(1) "horizontale" must be changed to "slightly aslant to the lower left;"

(2) the expression "le trait oblique supérieure" (sic!): the 2nd adjective is erroneously feminine (cf. also above, p. 4 );

(3) the tandem erroneously posits that that aslant line belongs to m, as he speaks of "...supérieure [sic] du m;" it belongs to k;

(4) the tandem also affirms erroneously that that very aslant line "dépasse un peu le trait vertical de cette lettre." This "un peu" equals the length of the upper aslant stroke of m!

The end of the tandem's study is an oracular pronunciamento, viz

"Lire: / m ou, avec Müller, bkm, ou bklm."

The first reading is so aberrant that it does not deserve any consideration for it eliminates two short, clearly traced lines readily acknowledged in the introduction, as shown above, in facsimile C compared with A. But, the publication of so unqualifiable a reading rejecting the existence of two obvious lines is totally despicable; cf. already, above, p. 6.

The second reading refers to "Müller" who is listed twice in the bibliography of the calligram. Which reference is involved? Drewes-Schneider does not say! The reader, thus, has no choice but to search for the answer himself.

The first place is J.Th. Bent, The Sacred City of the Ethiopians..., London, 1896, p. 238, Yeha 9: "bkm." For the erroneous date of "1893" given in Drewes-Schneider's bibliography, cf. already above, p. 4.

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above, p. 5). Vienna, 1894, p. 62, Yeha IX: "bkm?." For the Drewes-Schneider tandem there is, thus, no difference whether the name is followed by a question mark or not, viz the question mark has no value whatsoever and means nothing!

The third reading is "bklm." Cf. also RIE 64 (p. 144 and pl. 51, upper left corner).

It looks as if the discovery of the third reading actually exhausted the Drewes-Schneider tandem, depriving him of any further stamina. It does not, however, require any special effort to read kbm or even km instead of "bkm." But, these two alternatives are not even suggested. Furthermore, the name "bklm," although epigraphically attested (cf., e.g., Qat Ja 316), is nonetheless discarded in favor of another name that immediately comes to mind as much more frequently and more widely mentioned, viz ← klbm Kalbum.

Cf., e.g., Ḥaḍr CaTh 67 b/1, Qat Ja 244, and Sab Ja 573/2.

Finally, the Drewes-Schneider tandem's statement on "la copie de Salt, qui est reproduite ici" (cf. also p. 14 and fn 18) is the last stroke of the pen showing the deviousness of the two coauthors' argumentation. Their affirmation is grossly erroneous because the tandem refers exclusively to P.-F. Henry's French translation of H. Salt's book.

1 - The facsimile of the calligram reproduced by the tandem (cf. above, p. 8, A) does not come from the French translation, which is B. The four main differences between the two calligrams A and B are as follows:

a - the seven lines composing the calligram are all straight in A, but 4 of them are undulated in B, viz the two vertical bars and the lower and third bars of m;

b - the width of the calligram is, respectively, 1.1 cm in A, but 1.6 cm in B;

c - the width of the stone is, respectively, 2.5 cm in A, but 3.1 cm in B; and

d - the upper aslant bar of m and the aslant bar of k do not touch the upper left corner of b in B, but they do in A.

2 - The facsimile reproduced by the tandem does not come from any of the five references listed in p. 122; none of them reproduces the design of H. Salt's English book. Therefore, how could it be possible to understand such incredible an imposture? The Drewes-Schneider tandem actually reproduces the facsimile of H. Salt's English volume while, at the same time, refusing to utilize it and exclusively adopting the French translation which published a counterfeit reproduction, thus never drawn by H. Salt, of the latter's original design. What a childish chauvinistic attitude! But, the tandem did not innovate: he simply walked in the footsteps of F. Anfray who already used the same subterfuge (cf. below, p. 14) and authors RIE's "Introduction" (pp. 19-65).

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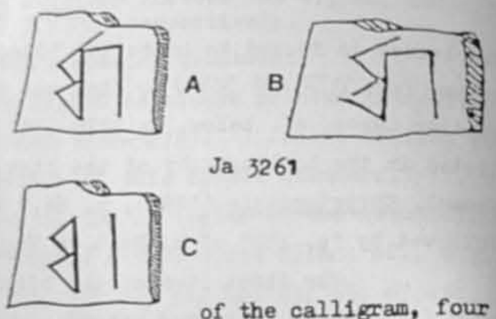


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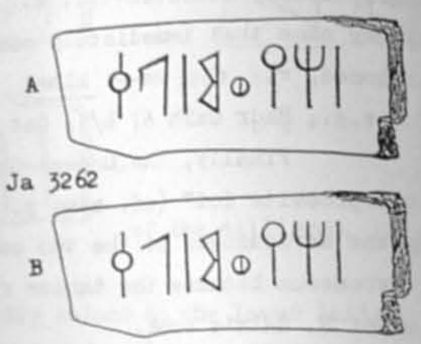
2 - The facsimile reproduced by the tandem does not come from any of the five references listed in p. 122; none of them reproduces the design of H. Salt's English book. Therefore, how could it be possible to understand such incredible an imposture? The Drewes-Schneider tandem actually reproduces the facsimile of H. Salt's English volume while, at the same time, refusing to utilize it and exclusively adopting the French translation which published a counterfeit reproduction, thus never drawn by H. Salt, of the latter's original design. What a childish chauvinistic attitude! But, the tandem did not innovate: he simply walked in the footsteps of F. Anfray who already used the same subterfuge (cf. below, p. 14) and authors RIE's "Introduction" (pp. 19-65).

2 - Ja 3262 = RÉS 2629 = RIE 42 (p. 121).

Contrary to Ja 3261 (cf. above, p. 7) and most probably in an effort to prove the Drewes-Schneider tandem's consistency, the reference to RÉS 2629 is given in the introduction. But, here again as in Ja 3261, the exact location where H. Salt found the stone and the upside-down position of its facsimile are not mentioned. H. Salt's design printed upside down in both the English volume (p. 431) and P.-F. Henry French translation (II, p. 210) is reproduced in RIE = A (below), while B shows the mutilated appendiculated penult as in D.H. Müller's sketch, which is endorsed by the tandem.




The last phrase of the tandem's study reads as follows: "On lit d'après Salt et Müller:

/ ḫywm/ ḳq".

This statement erroneously telescopes two different realities, viz Salt's original design (with no reading) and Müller's partial copy (with partial reading). Again, the tandem does not specify where Müller gives his spurious sketch. As in Ja 3261 (cf. above), it is in J.Th. Bent, l.c., p. 238, Yeha 8, that Müller gives the above-mentioned sketch B. In his Epigraphische Denkmäler..., p. 62, Yeha VIII, Müller eliminates the introductory word divider; thus, his design is doubly incomplete. Incidentally, the tandem deserves to be congratulated for resisting C. Robin's ceaseless propaganda on behalf of his demented transcription system which, among other things, eliminates all word divider (cf. already denounced in JaMAR, X [1979], p. 43). Here again, however, as illustrated by facsimile B (cf. above), the tandem endorses Müller's mutilation of the penult, which discards the lower left appendix of the letter; the phoneme of Salt's design is ġ (cf. l.c., p. 48). However, could the pointedly curved shape of that appendix represent a hole or crack in the stone? Such a hypothesis is lacking in any support. H.Salt's testimony (cf. l.c., p. 432) leaves no doubt on the perfect distinctness of the engraving, which manifestly excludes any hole or crack in the stone, viz

> "These characters were very boldly carved, and they appeared to be as fresh and perfect as on the day they were executed, two of them being cut into the stone, and the other raised above the surface."



Incidentally, here follows P.-F. Henry's translation (cf. II, p. 210) where the 2nd half of the English text, viz the participial sentence, is eliminated, viz

> " Les caractères de ces fragmens ont été gravés avec beaucoup de hardiesse; et ils paraissaient aussi entiers, aussi parfaits, que le jour où l'artiste les avait finis."

Anyone tempted to reject the appendix of the penult as an adventitious addition should in all honesty first deny the authenticity of the five designs published by H. Salt, but he could not take such a step before proving that H. Salt is a forger. The insanity of such a move is amply demonstrated by RIE, II, pl. 33, lower left, which is the photograph of the longest facsimile made by H. Salt, l.c., p. 432, top = P.-F. Henry, II, p. 210, center.

The text may be read as follows: /ḫywm/ġq

Haywum [of the family] Ġaqq.

ġq is found in Saf LP 953 and Wi 418.

3 - Ja 3263 = RIE 319 (p. 426, and photograph in pl. 199, top).

This ostracon of Matara was unearthed in "B, au nord-est du monument, au pied du mur extérieur." There is no bibliography to enlighten the reader. The whole p. 54 is devoted to an amateurish, approximate sketch of the "Site archéologique de MATARA" which is a mute, useless skeleton because none of the 15 alpha-numerical signs, A - I2, indicating places and buildings on the sketch is mentioned in the summary of the findings at Matara in pp. 55-57, and only 10 out those 15 marks are found referring to the general areas where the inscribed shards come from, viz RIE 315 (p. 422) to 383 (p. 475), thus forcing the reader to turn pages after pages, up to 8 pages (from p. 475 to p. 467) to find an information not related to the sketch of p. 54; and none of the 75 RIE numbers gives any information on the nature of the places themselves. Obviously enough, the tandem's note on "B, au nord-est...," refers to the sketch of p. 54. Unfortunately, nowever, "au nord-est du monument..." is not intelligible because the "northeast of the monument" does not exist: the whole upper right corner is a blank. So, where is it?

Let us take the first four Matara numbers, viz RIE 315, 316, 317 and 318, the example given above is RIE 319.

RIE 315 (p. 422): "De A, au pied du tertre, à l'ouest;"

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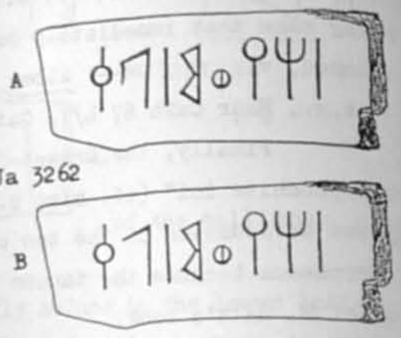


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Let us take the first four Matara numbers, viz RIE 315, 316, 317 and 318, the example given above is RIE 319.

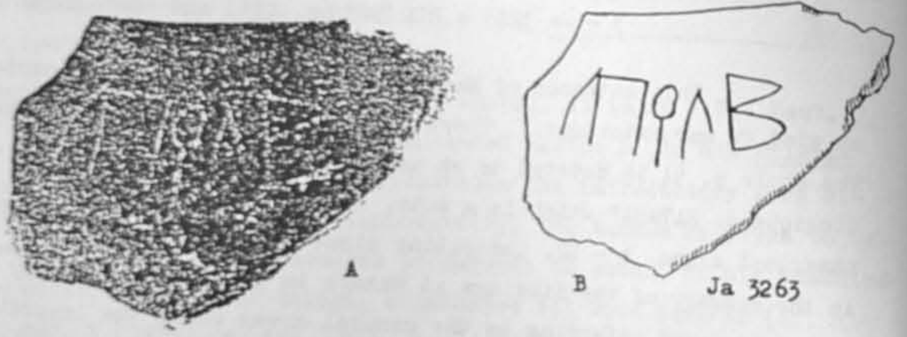
RIE 315 (p. 422): "De A, au pied du tertre, à l'ouest;"

316 (p. 423): "De A, loc. 14;"

RIE 317 (p. 424): "De A 1, au nord-est du tertre de ruines" and
318 (p. 425): "De B, au sud, à l'extérieur du monument."
These pieces of information are much too vague to be of any help, and worst of all, "A, loc. 14" and "A 1" do not even exist in the sketch. Another example of this kind of reference is Ja 3278 (cf. below, p. 38).

The introduction informs the reader that the inscription is "gravée sous le bord." This is obvious: where else could it be? The shard, as part of a bowl, has no neck. It would, thus, have been appropriate and useful to note that the name is engraved in the horizontal axis of the fragment. The tandem adds that the engraving is "apparemment complète à droite." And on the left? The answer is given at the end of the "Commentaire" (cf. below). The tandem gives the height of both m and y, apparently the two extremes in height, as 1.4 and 1.7 cm. In fact, they measure 1.5 and 1.8 cm, respectively, and the shortest letter is l with 1.4 cm.

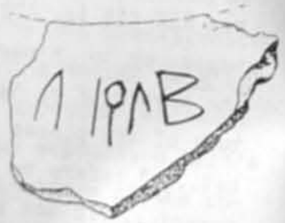
The photograph of RIE 319 is very grayish and is reproduced below, A; but the name is still readable; my tracing from that photograph is B, and C reproduces RIE's reduced facsimile. The author of this design remains unknown. P.14 lists the names of the 12 persons involved; "Yves Baudouin du service de dessin de la Mission française d'archéologie, en Éthiopie" is attributed "une grande partie des dessins," while fn 10-19 list the numbers contributed by 10 other persons. The 10th person of the list, however, "Isabelle Pierre," surprisingly enough is not given any number!!! Thus, unless the number/numbers drawn by Pierre is/are known, the author of any facsimile not listed in fn 10-19 remains uncertain unless, of course, the facsimile be accompanied by some identification mark, e.g., "YB" in RIE 180 = JE 5. Note that this facsimile eliminates (again!!!) the small circle inside r of gdr, that is clearly attested in LeMAA, pl. VIII; RIE's photograph of pl. 89, bottom, is perfectly useless (cf. already above, p. 6). Obviously enough, the elimination of that undeniable circle by a professional drawer, i.e. a person who by training specializes in seeing and reproducing minute details, makes the omission of the central horizontal bar of ġ of the present name the signature of the whole group although Y. Baudouin is not considered as a member of the "équipe" by whom "ce recueil a été préparé" (p.12).



A



B Ja 3263



C



The introduction ends with this phrase:

"On lit, de gauche à droite:
ṯ / ylm".

The Drewes-Schneider tandem adds this:

"Commentaire :

Les signes sont beaucoup moins serrés à gauche qu'à droite; il n'est donc pas exclu que l'inscription soit incomplète à gauche. La lecture du premier signe n'est pas sûre : s'il s'agit effectivement d'un *l*, il diffère sensiblement de l'avant-dernière lettre; pourrait-il s'agir d'un symbole? En ce cas il marquerait le début du texte."

Cf. above, p. 5, for a note on the question of the tandem's "Commentaire." - This commentary could not be worst: it amply proves that A.J. Drewes and R. Schneider have no idea of what they are talking about. The straight line joining together the tops of the two slightly aslant lines on the left is as obvious as can be. The two co-authors cannot deny seeing that long horizontal line because they could not do otherwise than see it. Yet again, they voluntarily ignore it. This is a repeat of the case of Ja 3261, facsimile C, and 3262; cf. above, pp. 8 and 10, respectively.

The name is complete, obviously. The distance between the letters varies between 0.15 and 3 mm on the above-reproduced photograph; and the empty spaces left and right of the name are 5.5 and 8 mm, respectively.

The dextrograde name reads as follows:

→ ġylm Ġaylum.

ġylm, cf., e.g., the Sab proper name in Gl 912, and dġylm, name of a city in both Ḥaḍr Ja 2888/4 and Qat RÉS 3688/11. - The extreme left letter ġ is sinistrograde in a dextrograde name; the last two letters, l and m, are dextrograde; they are thus running in opposite directions. The direction of these two letters and the very strong onomastical parallels ascertain the reading of ġylm, leaving no practical possibility of reading the sinistrograde mlyġ, which would be derived from the roots lyġ or mlġ. For the shape and position of l, cf. below, p. 69: Nationalmuseet 14646b.

C - The open-work metal marks.

The collection of open-worked metal marks from Ethiopia, both stamps uti-

lized to print seals similar to Ja 3261 (cf. above) and two appliqués, is presented in RIE, I, pp.401-413, under the title of "Les petits objets inscrits," viz 17 items in nos. 292-308, and II, pl. 186-192 with 16 items, nos. 292-306 and 308.

The following pages herald in the full treatment after the publication of the commentary of vol. III... if and when the Academy releases it.

1 - Remarks needing no epigraphical inquiry.

a - The above presentation of the illustrations of II already introduces the reader to RIE's characteristic disorder: there are 16 numbers from 292 to 308, but no. 307 is missing; and two reproduced items (= Ja 3268 and 3271) are not identified, they are not even mentioned.

(1) The photograph of RIE 307 = Ja 3277 (cf. below, p. 40) is not reproduced in pl. 192 although the item is "maintenant au Musée National à Addis Ababa." How is it possible that the French team did not as yet photograph an item unearthed by them in February-March 1960 (cf. AnCFY, pp. 171, 172, 185 and 186), thus more than thirty years ago? And they practically run the museum! It should be added that AnCFY also refers exclusively to the French translation of H. Salt's A Voyage... without even acknowledging that it is not the original and mentioning the translator's name; cf.p. 171, fn (2), which reads as follows:

"H. SALT, Voyage en Abyssinie, Paris, 1816" (cf. already above, pp.7 and 9).

(2) The non-epigraphic JE 1833 and 2114 are represented as RIE 293 and 295 in pl. 186, center, and 187, top, extreme left.

(3) The epigraphic Ja 3271, 2nd from the right in p. 187,top,is not given any number.

(4) The epigraphic Ja 3274 represented in pl. 188, top, lower left corner, is not given any number.

It baffles logic to reproduce intact two composite photographs of which one section of each is discarded from the study.

(5) The last item of both pp. 404 and 413, RIE 309, does not belong to the stamp collection. The whole introduction (pp. 403-404) devotes 57 lines to "objets en bronze ajouré" (p. 403, l. 3), but the last two lines of p. 404 introduce RIE 309, which is "une [sic!] fragment de lamelle en bronze" (p. 413). Contrary to the leitmotiv of p. 403 (reproduced above), "bronze ajouré," RIE 309 is "en relief" (in both pp. 404 and 413)! Cf. below, p. 46, the stranger case of RIE 291.

b - Most of RIE's introduction (pp. 403, l.8 - 404, l. 54) is the text published by the same tandem in DrScDEE, pp. 93-94, but revised in several places, especially in the 2nd half, including some new variant expressions; e.g.,



e.g., "Les noms propres inscrits dans les objets établissent que ceux-ci etaient personnels et privés" (cf. l.c., p. 94, ll. 27-29) and "les noms propres figurant sur certains de ces objets font penser que ceux-ci etaient personnels et privés" (cf. RIE, I, p. 404, ll. 42-43).

c - The introduction is very superficial, vague and deprived of any textual reference; e.g., what is the exact meaning of expressions, such as "une symétrie peu poussée" ou "une symétrie souvent très poussée"? For the tandem's meaning of "peu," cf. above, p. 8.

d - The first of the four places from which the stamps originate is Sabea (p. 405); for the three stores of the site, cf. LeMiRA, pp. 111-114, and LeMAA, p. 302. The name "Sabea" is neither explained among "Les sites archéologiques" (pp. 37-59) nor found in any of the six maps which follow the preceding presentation. In p. 403, fn 1, the tandem limits himself to one simple reference, viz "Pour le site de Sabea, cf. AE, III, 1959, pp. 110-111." This is LeMiRA mentioned above, but the pagination is wrong as p. 110 does not deal with "le site de Sabea" which is described in pp. 111-114: "Les [three] dépôts de poteries et de bronze à Sabéa." The six ill-conceived maps of pp. 60-64 must be redrawn to contain all missing place names because "Sabea" is not an exception; several other place names are not found in the printed maps; e.g. "Ruba Kodo" (8 times; pp. 172-176) and "Me'elan Debbas" (p.306). Yet, the whole p. 54 is devoted to a useless skeletonic sketch (cf. above, p. 11).

e - As if to make more evident the lack of planning and rational execution, the orthograph of the place names on the maps is very often different from that of the text; e.g.

"Addi Qayyih" (p. 157) and "Addi-Qayyeh" (pp. 61 and 62); and
"Dakhanamo" (nineteen times, pp. 197-208) and "Dekanamo" (p. 62).

Then, the reader is entitled to once more and again ask himself the question of how and where did the editor(s) and reviser(s) discharge their duties?

f - It is equally difficult to understand that the introduction of Ja 3268-3281 does not mention the JE numbers.This anomaly was already noted on Ja 3261.

g - LeMAA's photographs of RIE 299, 300 and 302-304 (pl. XVI) are much clearer than RIE's illustrations in pl. 188-191.

The preceding remarks already manifest a careless preparation and an amateurish execution; and the following study provides the reader with an apparently inexhaustible flow of mistakes and errors confirming the conclusions already evident from the first glance of RIE (cf. above, pp. 3 foll), viz arbitrary myopia, appalling ignorance of South-Arabian epigraphy, illogicality and superficiality.

2 - Study of the marks.

a - Preliminary remarks.

The collection of marks presented by the Drewes-Schneider tandem is dealt with below with the exception of JE 2340, 2353 and AnCFY, p. 233 *g*, of which the information published so far is much too fragmentary, and none of them is listed in RIE's concordance of p. 527A. For JE 2340 and 2353, cf. AnCFY, respectively, pp. 186 ("[petite marque d'identité]") and 187 ("[marque d'identité]"), and 233 *g*: a photograph too imprecise to be of any help. Note that the two items of AnCFY's p. 233 *g* and *h* are not mentioned in either the text of the paper or RIE; therefore, their JE numbers remain unknown although all the other letters of the same plate are duly listed in the paper, viz *a* in p. 181;
*b* in p. 184;
*c*, *d* and *e* in p. 180; and
*f* in p. 174.

b - General description.

(1) The tandem's museographic description of the marks.

The tandem's presentation of the marks is worth reproducing below in order to fully appreciate his contradictions, the vagueness of his expressions the meaning of which cannot be evaluated, his unwillingness to refer to examples illustrating his statements in spite of the very limited material involved. Instead of taking advantage of this general Recueil... to reassess their former writings and interpretations, the two coauthors limited themselves to blindly copy, with a slight revision here and there, their old ideas and vues of DrScDEE, pp. 93-94 (cf.already above, pp.14-15). Here follows the tandem's text arranged in two columns for greater facility of comparing the parallel affirmations.



"On peut distinguer deux catégories" (p. 403, l. 19);
"la surface est toujours plane" (p. 404, ll. 4-5);
[I: p. 403, ll. 19-25] [II: p. 404, ll. 1-4, 6-8]

"Dans la première
[a] on trouve des objets relativement grands et épais,
[b] d'une symétrie peu poussée,
[c] figurant des animaux sacrés ou mythiques
[d] parfois très stylisés.
[e] La poignée est assez grande.
[f] Les objets ... contiennent des signes alphabétiques,
[g] parfois disposés en monogramme.
[i] Il s'agit, semble-t-il, souvent de mots complets, sans doute des noms propres."

la seconde catégorie
[a] les objets ... sont plus petits et plus minces,
[b] d'une symétrie souvent très poussée.
[c] ils ont des formes géométriques. Certaines formes reviennent: petits cercles ou anneaux placés à l'extérieur, croix ou rayons inscrits dans un cercle, chevrons et éléments en forme de V [!?: by definition, "chevron" is "galon en forme de V renversé"].
[e] La poignée est petite et en forme d'anneau.
[g] On reconnaît parfois des monogrammes
[f] ou des lettres isolées;
[h] d'autres sont anépigraphes.

As all the letters of the name are found in each mark, the tandem should at least have used the expression "monogramme parfait." But, even that would have been inaccurate since the word "monogramme" does not imply the originality of the item, viz "to form a decorative pattern or figure (as for a seal)," which is the characteristic of a calligram. - Faced with such an accumulation of vague approximations, *e.g.*, "relativement grands et épais," "peu poussée," "très stylisée," etc, the reader cannot understand the tandem's apparent unwillingness to utilize and systematically complete the information given by the editors (including themselves) in order to substantiate and illustrate their empty affirmations. The reader, however, notes that the tandem's long description does not contain any cross-reference to p. 41, where F. Anfray knows only (What a simplism!!!) "marques d'identité" [with reference to DrScDEE's "pp. 89 ss." in p. 42, fn 3, where "89" must be corrected to "92"] "qui représentent" four animals; thus, RIE 300 and 302-304, which "sont des plaquettes épaisses ... d'une dizaine de centimètres." Incidentally, the stylized bull of RIE 300 measures only 5.2 x 2.1

cm according to RIE, I, p. 409. How could "5.2 cm" equal "une dizaine de centimètres"? The answer is simple: it lies in the fact that the tandem eliminated the length which is 8 cm, and understood "hauteur" to mean "length"! (cf. immediately below, table of measurements). What the tandem should have done is to collect the measurements of the marks and draw the necessary conclusions. Here follows the list of those measurements found in different publications, viz

(a) The marks themselves:

(1) In RIE which rarely complements previous publications, but often shortens and sometimes distorts them.

11 x 6.7 cm (p. 411) in Ja 3266; but 11 x 6.7 x 1.7 cm in AnCFY, p. 184; cf. haft, below;

11 x 6.2 cm (p. 410) in RIE 303; but 11 x 6.2 x 2 cm in AnCFY, p. 181; cf. haft, below;

10.3 x 3 cm (p. 408) in RIE 299; but 10.3 x 3 x 1.1 cm in AnCFY, p. 180;

8.7 x 6.6 cm (p. 410) in Ja 3265, but 8.7 x 6.6 x 1.3 cm in AnCFY, p. 181; cf. haft, below;

8.5 x 4 cm (p. 405) in Ja 3264; these measurements are not to be found in any of the two papers listed in the bibliography;

8 x 5.2 x 1.2 cm (p. 409) in RIE 300; RIE's figures are erroneously copied from AnCFY (p. 180): "Hauteur: 5,2 centimètres; longueur: 8 centimètres; epaisseur: 1,2 centimètre;" but RIE has "longueur 5.2 cm; hauteur 2,1 cm;" the figures to be read are: 8 x 5.2 x 1.2; cf. below, haft and also the study of RIE 295 for a similar confusion of measurements due to faulty terminology;

8 x 4.5 cm (p. 407) in Ja 3273; same measurements as in AnCFY, p. 176, cf. haft, below;

± 7.7 x 4.3 x 0.5 cm (p. 413) in Ja 3278; the first figure is approximative; it was restored on the basis of the "longueur conservee 4,5 cm;" same measurements in AnAnM, p. 86; cf. haft, below;

5.8 x 2.9 cm (p. 412) in Ja 3277: same measurements as in AnCFY, p. 186;

5 x 2.8 cm (p. 407) in Ja 3272; but 5 x 2.8 x 0.5 cm in AnCFY, p. 176; cf. haft, below;

4.8 x 2.8 cm (p. 406) in RIE 294; the measurements are not to be found in DrScDEE (p. 95) listed in the bibliography;

4.7 x 2.1 cm (p. 409) in Ja 3269; but 4.7 x 2.1 x 0.7 cm in AnCFY, p. 180; cf. haft, below;

4.2 x 2.7 cm (p. 411) in Ja 3270: same measurements as in AnCFY, p. 186;

± 2.8 x 1.2 cm in Ja 3268. In RIE (p. 408), the present measurements are 2.5 x 1.2 cm; the first figure is not to be found in AnCFY, p. 176; cf. haft, below and the commentary on the text;

2.4 x 1.3 cm (p. 412) in Ja 3276; same measurements as in AnCFY, p. 186;

diameter: 3 cm (p. 405) in RIE 293; no measurement in CoFH, p. 48 or in DrScDEE, pp. 94-95; cf. haft, below;



(2) Elsewhere:

width: 1.2 cm (cf. AnCFY, pp. 176 and 231 i) in Ja 3274; thus, the length is ± 3.3 cm;
3.7 x 3.6 (cf. l.c., p. 186) in JE 2340;
length: 4 cm (cf. l.c., p. 187) in JE 2353;
? ? (cf. l.c., p. 233 g), cf. below;
? ? (cf. l.c., p. 233 h) in Ja 3275;
? ? (cf. CoFH, pp. 44 and 69, bottom, no. 3) in JE 1713, cf. haft, below;
? ? (cf. DrScDEE, pp. 94 and 104, fig. 5) in Ja 3267 = JE 1839.

(b) The haft on the back.

(1) In DrScDEE (1967): "ces objets [= "Marques d'identité" of p. 92], tant de l'Arabie que de l'Éthiopie, présentent tous une prise au dos" (p. 93). This short note introduces the study of 16 stamps (pp. 93-96), which are RIE 292-307, but is not repeated each time. At the time of its writing, this note was already disproved by JE 2882 = Ja 3278 (cf. below, pp. 37-38). Yet, it is still repeated by F. Anfray in RIE's "Introduction" (p. 41): "Tous [= "Ces objets" = "marques d'identité"] possèdent une attache rigide au dos" in spite of JE 2882 discovered more than 30 years ago, probably in 1962, according to AnAnM (1965), p. 86, who recognized the item as an "applique." This identification was acknowledged by R. Schneider himself in AE, 6 (1965), although in contradictory terms, viz "une applique (?)" in p. 91, but "Applique" in p. 142, fig. 4. This apparent contradiction of Schneider's paper comes from his paper using some AnAnM's illustrations. This acknowledgment did not prevent the same Schneider from signing DrScDEE's above-quoted denial in AE's following tome 7 (1967). My date of "probably 1962" needs some explanation. AnAnM describes the three excavation campaigns of 1961, 1962 and 1963 (p. 49) as a whole, in a joint presentation without distinguishing the years, thus preventing the reader from knowing the year of each finding. It would have been so easy, so simple to introduce the "Table des objets" (p. 77) with a very short note stating this:

"JE ...-..., ...-... and ...-..., in 1961, 1962 and 1963, respectively;"

and the reader would have had no difficulty attributing each JE number to its proper year. Although there is plenty of spare space at the bottom of p. 76 to admit such a short, yet most valuable information, the obvious was not done. As it stands, the "Table des objets" (pp. 77-86) lists the numbers 2740-3076. But, "quelques objets d'intérêt particulier" (p. 75), of which some have "inscriptions de type sudarabique," are 9 numbers ranging from 2082 to 3721, not even listed in normal succession since 2809 follows 3026! Therefore, the reader does not even know either the exact numbers of the findings of each year or the year they were unearthed. The number 2882 being at the

middle of the two series provided by the two coauthors, F. Anfray and G. Annequin, it is attributed to the middle year, 1962. It may well be a blessing in disguise that the preceding information was not given when the reader considers the two following examples. AnAnM (p. 61) speaks of a strange finding, viz "un tesson ... inscrit de 44 lettres" and "du caractère réduit de l'inscription"! R. Schneider (cf. AE, 6 [1965], p. 91) corrects the preceding misprint with "quatre lettres" which became "quelques lettres," in fact five in RIE 320 (p. 427)! Furthermore, AnAnM speaks of "3,50 mètres" as the depth where the shard was found. This information became "3,60 m" in RIE, who endorses Schneider's gratuitous dextrograde reading of the identity card, contrary to the sinistrograde initial g. what a maze of confusion!

(2) In RIE:

p. 411, "en forme de croissant;" 3.5 x 2.4 [width] cm in Ja 3266; same measurements in AnCFY, p. 184;

p. 410, "en forme de croissant;" 4.8 x 2 [width] x 3 (height] cm in RIE 303; but 4.8 x 3 x 2 in AnCFY, p. 181; thus, the last two numbers were inverted;

p. 405, "la poignée;" 4.4 x 2.2 [height] cm in Ja 3265, but 4.4 x 2.2 x 0.8 cm in AnCFY, p. 181;

p. 408, "anneau de préhension" in Ja 3268; also in AnCFY, p. 176;

p. 413, "pas de trace de poignée ou d'anneau de préhension" in Ja 3278; cf. below.

(3) In AnCFY:

p. 176, "anneau de préhension" in Ja 3273; not mentioned in RIE, p. 407;

p. 176, "anneau de préhension" in Ja 3272; not mentioned in RIE, p. 407;

p. 180, "une prise" in RIE 300; not mentioned in RIE, p. 409;

p. 180, "une prise" in Ja 3269; not mentioned in RIE, p. 409;

(4) In CoFH:

pp. 48-49, "anneau de suspension" in RIE 293; not mentioned in RIE, p. 405;

p. 44, "anneau de suspension" in JE 1713.

(c) Remarks on the tandem's position.

Before going back to the tandem's summary, a few remarks on the preceding list are in order. The most obvious consideration is the flabbergasting shock of having to contemplate such an incoherent, disparate, shockingly lacunary and scanty presentation of the measurements of the items.

[1] No measurement at all on the last four items of the first list;

[2] one measurement only is available on RIE 293, Ja 3274 and JE 2353;

[3] RIE gives two measurements only although AnCFY gives the three dimensions on Ja 3265, 3266, 3269, 3272, RIE 299 and 303, and even the three dimensions of the haft of Ja 3265 and RIE 303;



[4] the tandem mentions the thickness of the openwork only once, in Ja 3278;

[5] the tandem eliminates without any justification the mention of the haft in Ja 3269, 3272, 3273, RIE 293 and 300;

[6] the reader must raise the question of any possible relation between JE 2340 and 2353 on the one hand and, on the other, the two photographs of AnCFY, p. 233 g and h.

Returning to the tandem's division into "grands" and "petits," the reader must emphasize that not a single clue justifying or explaining the tandem's partition is volunteered. It is simplistic to speak of "large" and "small" marks because the two adjectives are immediately applicable, e.g., to 11 x 6.7 cm (in Ja 3266) and 2.4 x 1.3 cm (in Ja 3276). The main question, however, is: where does the tandem draw the line ... in between? In fact, there are three groups of marks based on their measurements, viz

small: 2.4 (Ja 3276) → 5.8 (Ja 3277)

← 18 units are missing between 5.8 and ±7.7.

middle: ± 7.7 (Ja 3278) → 8.7 (Ja 3265)

← 15 units are missing between 8.7 and 10.3.

large: 10.3 (RIE 299) → 11 (Ja 3266)

The only possible help from the scanty information available so far is the thickness of the mark which is the same as that of the haft in RIE 303, viz 2 cm (cf. above, pp. 18 and 20). In an effort to make the best of the very little information gleanable in the editions, the following shema presents the thickness of the mark and also the proportion between the thickness and the length and the width of the mark. As it is immediately apparent, the results are unexpectedly significant and point to the difference between stamp and appliqué.

| category of the stamp | thickness of the stamp | siglum of of the stamp | measurements of the stamp in cm | proportion between the thickness of the stamp and the length of the stamp | proportion between the thickness of the stamp and the width of the stamp |
|---|---|---|---|---|---|
| small | 0.5 | Ja 3272 | 5 x 2.8 | 10 | 5.6 |
| | 0.7 | Ja 3269 | 4.7 x 2.1 | 6.7 | 3 |
| | [3 units are missing] | | | | |
| middle | 1.1 | RIE 299 | 10.3 x 3 | 9.3 | 2.7 |
| | 1.2 | RIE 300 | 8 x 5.2 | 6.6 | 4.3 |
| | 1.3 | Ja 3265 | 8.7 x 6.6 | 6.6 | 5.07 |
| | [3 units are missing] | | | | |
| large | 1.7 | Ja 3266 | 11 x 6.7 | 6.4 | 3.9 |
| | 2 | RIE 303 | 11 x 6.2 | 5.5 | 3.1 |

The preceding presentation applied to the only appliqué recognized so far gives the following numbers:

small (according to the tickness of the item), but
middle (according to its measurements)

| | | | | |
|---|---|---|---|---|
| 0.5 | Ja 3278 | ± 7.7 x 4.3 | 15.4 | 8.6 |

The value of the preceding chart depends on the accuracy of the figures which cannot be ascertained, and the reader does not know the exact places where the measurements were taken and whether these measurements are constant. The following conclusions may be drawn from the preceding schema.

[1] It should be emphasized that the tripartite division of the items is confirmed; this information removes all probability from the tandem's aprioristic division.

[2] RIE 299 has shifted from the "large" group to the "middle" one, and the thickness of Ja 3278 puts it among the "small" although its measurements had it in the "middle." But the two proportions are so much greater than in all the seven other cases that these exclude it from them. The thinness of the item is related to, and explained by, its purpose: the item is a sign, an appliqué of which the small size excludes it from being affixed to a door; it would hardly be recognized.

[3] The thickness of the mark is not linked to its measurements; the relation between these two must be sought above all in the destination of the item as well as in the making process and both the number and location of the weak points.

Therefore, the division of the marks on the basis of the measurements is too simplistic an approach to the problem of their classification.

Going back to the tandem's presentation of the marks (cf. above, p. 17), the reader can immediately see that it baffles all factual information and can only be understood as a dreamy expectoration unworthy of a detailed discussion. Only a few particular questions not included in the preceding chart are brought up here.

1 - Ad Ib = IIb. - The symmetry of the marks depends on the nature of the designs, as nobody expects it in a design profiling an animal, but geometrical designs and calligrams can be made symmetrical.

2 - Ad Ic = (without any cross-reference) "Ces animaux symbolisent évidemment des divinités du panthéon sudarabique" (p. 41). - The statement on "animaux sacrés ou mythiques" is nothing but a mere label or an empty shell still favored by mythmakers, such as J. Ryckmans, A.F.L. Beeston, W.W. Müller and company, based on the primitive, antiquated assumption that all designs or statues originate from, or



are caused by, religious beliefs ... as if the natives could not possibly draw the animals living in their vicinity as ornaments in their jewelry or writing. For the case of RIE 303, cf. below, p. 51. Then, surprisingly enough, the tandem declares himself unable to determine whether "un dessin abstrait ou [] la figuration stylisée d'un animal" is meant in Ja 3264 (p. 405). If the two coauthors are so infatuated with animal designs, how is it possible for them not to think of the stick figure of an animal where they fail to see a thing, viz two long snakes in Ja 3273 (p. 407), a fish in Ja 3264 (p. 405), and not to speak of the stick figure of a turtle in RIE 299 (p. 408) where they also talk of "un animal stylisé"? These three marks have in common that they would sketch the animals viewed from above. But, the ascertained stick figures of animals, Ja 3265, 3266 and RIE 300, are constantly profiling the left (not, the right) side of the animal; cf. also below, p. 133, the camel of Ja 3237.

3 - Ad IIc. - This description is a manifest exaggeration based on RIE 293 and 295 because "petits cercles" and "chevrons ou éléments en forme de V" (cf. above, p. 17) also exist in his 1st category; e.g., Ja 3264, 3265, 3266 and RIE 299.

4 - Ad Ie and IIe. - It would be quite ridiculous to add a large haft to a small stamp or a small haft to a large stamp. Ie simply forgets that the tandem speaks of "en forme de croissant" in both Ja 3266 and RIE 303, while hiding from the reader the position of that "crescent," viz is it linked to the stamp by its vertex or by its extremities? Furthermore, the circular haft of Ja 3273 is not even acknowledged by the tandem, who also prefers to ignore that he "ne voit pas de trace de poignée ou d'anneau de préhension" in Ja 3278.

5 - Ad If-g = IIf-g. - These two identical summaries would practically eliminate all difference whatsoever between the two categories. Yet, among the 10 largest stamps listed above (p. 485), only RIE 300 shows a calligram in the second personal name. RIE's erroneous confusion is illustrated by the descripion of p. 41, which speaks exclusively of the stamps with an animal frame, emphatically posits that "intérieurement [= inside the animal frames] des lettres sudarabiques qui épousent étroitement la forme de l'objet composent des monogrammes aux noms de personnes." Beside the gross exaggeration on the number of the calligrams, the last statement is another fallacy as if the letters were set up to snugly fit the chosen frame.

6 - Ad Ii = no counterpart in II. - Each epigraphical stamp belonging to either categories contains a personal name.

(2) The tandem's interpretation of the nature of the marks.

The tandem ends his descriptive presentation of the marks stating that "la signification et l'emploi de ces objets sont inconnus" (p. 404), but the interpretation of them as "marques d'identité," which was already suggested by AnCFY, e.g., p.

174, "n'est pas forcément correcte" (cf. l.c., l. 20). This strictly negative statement ends a paragraph and, therefore, the reader is left with his frustrated expectation of learning a new revelation based on their experience of almost 40 years of field-work. There is not even a cross-reference to F. Anfray's exposé of p. 41, which reads as follows:

> " Il est possible de les interpréter comme des pièces d'identification, des amulettes magico-religieuses, des timbres à imprimer des marques colorées sur des tissus ou sur des parchemins."

Here again, no consideration, justification or remark aims at corroborating any of these suggestions. The first interpretation is the identity understanding viewed from an outsider's viewpoint. The second definition comes from the author's religious understanding of the animals (cf. above, pp. 22-23) which he listed only seven lines above in the same paragraph and proves that, for him, the other marks do not exist. Finally, the third solution is disproved even at first sight by the absence of any dried substance reported on the basis of the stamp and not on the haft. It would be sheer madness to see a " magico-religious charm," e.g., in Ja 3269, 3270, 3271, etc. More importantly, F. Anfray does not even refer to any of the following theories proposed by three writers of his own group. The first was authored by J. Leclant - A.Miquel, viz "vraisemblablement des appliques de portes ou de selles" (cf. LeMiKA, p. 112) = "vraisemblablement des appliques" (pp. 124, lower left corner, and 125, upper left corner). In any event, the saddle is unacceptable because there is hardly any flat surface in any of them, and any stiff bronze appliqué would only wound human flesh and rend garment. But, the second suggestion of those marks as simple "Plaquettes à inscriptions stylisées" also came from J. Leclant (cf. LeMAA, pl. XVI). This is a mere description which does not deal with the purpose. Finally, the third opinion originates from CoFH (1963), who speaks of "anneau de suspension" (cf. text below, p.24, note on JE 1833"). For the general question of the haft, cf. above, pp. 19-21.

(3) General presentation of the marks.

The basic difference between stamp and appliqué is the presence or the absence of a haft affixed to the back of the item. The tandem pays a scanty attention to the haft: he refers to it in 5 cases (Ja 3265, 3266, 3268, 3278 and RIE 303); but he fails to do it in the remaining 12 items (Ja 3264, 3269, 3270, 3272, 3273, 3276, 3277, RIE 293, 294, 295, 299 and 300). Also significant of the tandem's superficiality is that he indiscriminately mixes the photographs of the flat side (opposite to the haft) in 13 items (Ja 3264, 3265, 3266, 3269, 3270, 3271, 3276, 3278, RIE 293, 294, 299, 300 and 303) and those of the haft side in 4 items (Ja 3268, 3272, 3273 and 3274). Yet, the preceding distinction is essential as shown by the following two remarks.

(a) The names of the 2nd series are those that will be seen on the plaster/wax seals, whereas the names of the 1st series must be reversed to be those of the seals.



(b) There cannot be any question of applied marks anywhere because the haft would prevent the fixing of the item on a flat surface as a hunchback cannot simply lie down flat on his back. And what might be the purpose or advantage of suspending an openwork by its center and having it dangling in the air? Finally, many of them would hardly be recognizable and readable against the dark background as doors and saddles normally have because of their small size; e.g., Ja 3276 almost equals the horizontal 2/3 of an ordinary American postage stamp, viz 2.4 x 1.3 against 2.5 x 2.1 cm. Summarily, those items were stamps easily held and manipulated by their haft and pressed upon soft material, such as wax or plaster; cf., e.g., the plaster seals found in Wâdî al-Jûbah (Ja 3155 ag A-D) and some others in the Yemen Museum (Ja 3160-3168 and 3255); cf. also, e.g., the bronze stamps (Ja 3260 a-g) published by W. Raunig in Yemen (1988), p. 410; each of these 7 stamps has an elongated plaque on the back, to which the haft is affixed.

The differenciating element among the stamps is the presence or the absence of a frame because a frame allows the making of a stamp with separate letters whereas a calligram has enough compactness to stand alone without a supporting frame. The symmetry of a name inside the frame depends on its letters; e.g., the symmetry is normal in Ja 3269, 3270, 3278 and RIE 294, but impossible, e.g., in Ja 3264, unless the name is repeated at least twice as, e.g., in Ja 3271. Symmetry is not synonymous of equilibrium or balancing (cf. already above, p. 22). Two differences of lesser importance are whether the frame is geometrical (e.g., RIE 294) or profiles an animal (e.g., Ja 3265), and whether the frame contains the calligram (e.g., Ja 3274) or is the center of the whole item (e.g., Ja 3273).

Out of the 22 items studied below,

3 are given a wrong interpretation, viz Ja 3265, 3269, 3270;

9 are not given any interpretation although some letters are read in each, viz Ja 3264, 3266, 3268, 3272, 3273, 3276, 3277, 3278; RIE 294;

4 were wrongly eliminated, viz Ja 3267, 3271, 3274, 3275;

2 others were also eliminated, but the absence of any description and/or photograph makes it impossible to figure out whether they are epigraphic or not, viz JE 2340 and 2353;

1 was eliminated, AnCFY, p. 233 g (cf. above, p. 16);

2 were erroneously taken as epigraphic, viz RIE 293, 295;

1 was correctly eliminated as non-epigraphic, viz JE 1713.

c - The marks according to both the frame and the symmetry of the letters.

All the marks dealt with below are constantly referred to by the Drewes-Schneider tandem as "Objets" (cf. the introduction of pp. 401 and 403-404), and

"L'objet" is the opening word in the presentation of each item. The two coauthors' insistence of using the word "objet" - it would have been difficult for them to find a word more general and more indistinct, a better passe-partout - reflects very well their frame of mind, viz they do actually not have any idea of what those stamps might have been used for.

(1) Asymmetry of both the frame often profiling an animal and the separated letters inside.

(a) Ja 3264 = JE 123 = RIE 292 (p. 405 and pl. 186, top).

The elliptical frame contains two hooks at one extremity, which combine together the three elements of the left of RIE 299, a large half circle at the opposite end, as in RIE 299, and one small chevron V in the center of each lateral side; for two series of V bordering the central element, cf. non-epigraphic RIE 295 (cf. below, after Ja 3278, pp. 39-40). RIE repeats AnCFY's error: "deux h dont un est renversé." In South-Arabian, an upside-down h is s. RIE adds: "peut-être un n ou un r" instead of AnCFY, "une espèce de n." No letter r ever presents an undulated central section. The name may be read as follows:

Ja 3264

→ (as n) hsn Hawsân.

Cf. the tracing on the right; the stippling in the center indicates the location of the haft support.

(b) Ja 3265 = JE 2220 = RIE 302 (p. 410 and pl. 190, top).

LeMAA's photograph (pl. XVI, lower left corner) is clearer than RIE's one. - Two particularities of the frame profiling an ibex (cf. already AnCFY, p. 181) should be noted, viz the tail is curved inwardly to form a semicircle that parallels the left horn, and from the center of the ibex's belly hangs the well-known V with the top pointing downward, opposite to, e. g., Ja 3264. - The tracing is reproduced on the right. - The two letters ʾn run in opposite directions. The tandem reads the name as "de droite à gauche ḥnʾt." In fact, this is the dextrograde (from left to right) reading of the imprinted seal, as suggested by ʾ; this direction is supported by Ja 3266, RIE 299 and 300. Nonetheless, I retain preferably the sinistrograde reading supported by n and Ja 3264, 3267, 3268 and 3276, viz ← tʾnḫ Taʾannaḫ. Both roots ḥnʾ and ʾnḫ exist. As the onomasticon is much too limited to favor a choice over the other, there remains only to retain the ductus more frequently attested.

Ja 3265

Two more questions need to be mentioned here.

(1) DrScDEE (p. 95, no. 14) refers to "ḥnʾ" in RÉS 3570 because "la restitution ḥnʾ,



avec h au lieu de ḫ, y paraît certaine." Whether the manuscript of vol. III reproduces this remark remains to be seen. At any rate, there is no restitution at all; it is known since 1909 that the letter unquestionably is ḫ; cf. D.H. Müller in both Anz. A.W.W., Ph.-hist. Kl, 1909, p. 6, and Mélanges Hartwig Derenbourg, Paris, 1909, p. 1, and F. Hommel, in OLZ, 12 (1909), col. 59. RÉS's error was still copied by A. Capuzzi's worthless Iscrizioni minee, Naples, 1974, p. 101, although K. Conti Rossini, Chrestomathia arabica meridionalis epigraphica, Rome, 1931, p. 77, no. 66/1, had faithfully reproduced ḥnʾ.

(2) RIE's pl. 190, bottom, which remains without any explanation, may represent the whole stamp seen from the left side. It looks as if the pairs of legs of the ibex are attached to the center of ʾ and n, respectively. One point, however, needs clarification. On the right extremity of the left side of the stamp itself, there seems to be two letters which are more visible in RIE's photograph than in AnCFY's p. 232 a; that section is not clear enough to suggest any interpretation.

(c) Ja 3266 = JE 2247 = RIE 304 (p. 411 and pl. 191, bottom).

Here again, LeMAA's photograph, p. XVI, upper left corner, is clearer than RIE's one. - The expression "un lion rugissant" comes from AnCFY, p. 184. - Each leg is composed of two elements, viz a well-rounded V (cf. RIE 299) to which a small, filled circle (cf. Ja 3274) is appended. - Here again, as in Ja 3264, the tandem confuses h and s in their statement on "deux fois hr." In the question of his "r" "à la gauche," the Drewes-Schneider tandem gives another proof of discretionary myopia. (a) The bottom of his "r" is well curved to the right to rejoin the next vertical line; (b) the section connecting the top of his "r" to the same vertical line joins together the two large curves profiling the back of the animal. - The tracing is reproduced on the right; here again, the stippling indicates the location of the haft support. - RIE erroneously speaks of "quelques signes, séparés semble-t-il par un trait à travers le milieu du corps de l'animal." It simply is the bottom of the plaque supporting the haft.

Ja 3266

The name may be read as follows:

→ (as r) srfh Sarfuh.

(d) RIE 300 = JE 2192 (p. 409 and pl. 189).

Here again, LeMAA's photograph of pl. XVI, lower right corner, is clearer than RIE's one, but does not show the haft support. - Also, the tandem gives an information useless to the reader, viz the origin of the mark is the "tombeau 6, caveau A" (same as in RIE 299 and 301). Where are those places? The reader has no way of

knowing since there is no sketch of the Yeha excavations and especially since those tombs are "sur le versant est de la colline du temple" (p. 40) while LeMAA (p. 301) places them "au Sud du temple antique;" for the summary of the Yeha excavations, cf. pp. 39-42. As indicated above (p. 18), the Drewes-Schneider tandem shockingly distorts without a single word of explanation AnCFY's testimony on the measurements (p. 180):

RIE 300

height: 5,2 cm; length: 8 cm; thickness: 1.2 cm; these figures became height: 2.1 cm; length: 5.2 cm in RIE.

As it is a stamp, the reading of the name is in the opposite direction as that of the imprinted seal, which is dextrograde, contrary to RIE, who reads the name as mṭs, as in DrScDEE, p. 95, no. 6, with the comment that that root does not exist in Geʿez or South-Arabian. – The letter ṭ was not retraced in black to make its form more visible. The name may be read ⟶ mṭs Māṭis.

Cf. Arabic maṭasa "smacked, cuffed;" cf. H.A. Salmoné, An Advanced Learner's Arabic-English Dictionary, Beirut, 1972, p. 986A, and G.W. Freytag, Lexicon arabico-latinum, IV, p. 190A; none of these two publications is to be found in RIE's bibliography, pp. 537 and 532, respectively.

(2) - Symmetric frame with asymmetric calligram.

Ja 3267 = JE 1839 = DrScDEE, p. 94, with the mention of the four small circles and, erroneously, of "chevrons" V ; no description on p. 95, and photograph in p. 104, fig. 5: the stamp is lying in its left side. - A rectangular frame with a small circle attached outside at each extremity of the small sides; cf. also a similar type of frame in Ja 3274 (cf. below) and JE 1834 (cf. CoFH, p. 49 and photograph in p. 80, top, no. 2; cf. below, remark on RIE 293). - The tracing of the calligram is reproduced on the right. - This stamp is discarded by RIE, who has no entry for JE 1839 (pp. 526B-527A).

Ja 3267

The name may be read as follows: ← (as n) qtn Qattân.

(3) Symmetric frame with separated letters.

(a) Ja 3268 = JE 2151 = RIE 298 (p. 408, and photograph in pl. 188, top, upper left corner). - In AnCFY, p. 176, correct "CLVI, 1" to "CLII, 1." Cf. the last remark on Ja 3272 on the origin of the photograph and the position of the mark. - Judging from the remaining fragment, the frame is concave (cf. below, the measurements), not rectangular, as claimed by RIE, p. 408, but nothing in both AnCFY, p.



176, and DrScDEE, p. 95, and plain, viz deprived of any ornamentation; the stippling inside the stamp shows the location of the haft support. This stamp is another example illustrating the presence of the haft hiding not a very interesting part of a central letter, as in Ja 3272, but the crucial section of the central letter of the stamp which would have been known with certainty had the photograph been taken from the other side. It is, of course, much easier for the photographer to set the stamp flat on a stand. That short moment of immediate satisfaction is nonetheless detrimental to research which is doomed to uncertainty ... until a good photograph is published... but when? What was intended to make research more certain is precisely what prevents it from achieving its goal. Furthermore, the two extreme letters point to opposite directions. Finally, the two lower extremities of b remain unconnected to each other; the letter ḏ is thus excluded.

Ja 3268

The name may be read as follows: ← (as r) ʿr¹(b)n Ḥa(bb)ân.

The tracing of the stamp was attempted in spite of two major difficulties. (1) RIE's measurements are difficult to assess because the tandem never says where he took those measurements; e.g., "largeur 1,2 cm." On my enlargement, however, the width is 4.8 cm across the haft, but 5.3 cm at the vertical axis of n. (2) The lower right extremity was slightly bent downward; it thus had to be straightened out. Here again, the rounded ellipsis with stippling in the center shows the location of the haft.

(b) Ja 3276 = JE 2338 = RIE 306 (p. 412 and photograph in pl. 192, top).

An elliptical frame, as in Ja 3275 (cf. above). Cf. the tracing on the right; the stippling indicates the location of the haft. - RIE identifies "un n, et un r de chaque côté, l'un senestrogyre, l'autre dextrogyre." This is a simplistic and superficial analysis of the stamp. It is obvious, indeed, that the two extreme letters cannot possibly be identical because the empty blank is circular on the right, but triangular-like on the left. The letter on the left is r, but the letter on the right must be read ʿ, and its left side is curved to exactly match the right which is a section of the outside frame.

Ja 3276

The name may be read as follows: ← (as n) rnʿ Rânîʿ.

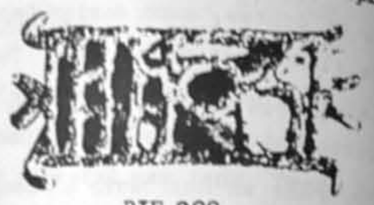
Although the two extreme letters are not identical, the name is still symmetrical because of the symmetry of the two extreme signs inside the frame. The letter r is dextrograde in a sinistrograde (cf. n) name. The dextrograde reading of the name as "ʿnr" is not suggested; the root ʿnr does not exist, and the nominal formation ʿqtl

with ʿ = ʿṯtr is so rare that it cannot be taken into consideration unless unmistakably attested.

(c) + adaptation to circular ʿ.

RIE 299 = JE 2191 (p. 408 and pl. 188, bottom).

On the location where the item was found, cf. above, the initial remark on RIE 300. - Here again, LeMAA's photograph (pl. XVI, upper right corner), which is reproduced here, is even clearer than that of AnCFY's p. 233 c; e.g., it shows distinctly what RIE's enlargement does not show well, viz the contiguity of the first 3 letters.-RIE 299 actually belongs to A, the group of framed stamps composed of separated letters although the letters ʾʿḏ are contiguous to each other.



RIE 299

They could hardly be anything else, one of them being ʿ that had to be connected to at least one neighboring letter and anchored on the haft support to stand; it could not be bound with the longitudinal stiles without becoming ḏ. RIE 299 actually illustrates the intrinsic deficiency of the processus using separated letters, viz the two circular letters w and ʿ cannot be used at all, even as the first or the last letter of a name; being hooked up only to the lateral side of the frame, the letter was not fastened strongly enough to remain in place. - For the measurements, cf. above, pp. 18-21. - The frame is understood as a "motif géométrique" by AnCFY, p. 233 c (but nothing in p. 180), "un animal stylisé" by RIE who fails to attempt any identification (cf. above, pp. 22-23). - For the reading of the name, cf. DrScDEE, p. 95, no. 11.

The name may be read as follows:

→ (with sinistrograde ʾ) ʾʿdd ʾAʿdad.

AnCFY erroneously speaks of the "racine ʿdd," which does not exist, and claims that "ʿdd" is known only twice in South-Arabian, viz RÉS 2639 and 3519. The second word of RÉS 2639 B is ʿddm, a qttl (reduplication) form derived from the root ʿdm. The beginning of RÉS 3519/1-2 = 5028/1-2 = Ja 881 A/1-2 reads ʿ(b) dkbr(2)n: the reading of the second letter as d is disproved by the fact that the letter is much narrower than any of the two d's of the text. The author continues his show stating that "il vaut donc mieux rattacher ʿdd à cette racine," viz ʿzz. This equivalence has already been suggested more than one hundred years ago by F. Hommell, Süd-arabische..., 1893, p. 133A. None of AnCFY's preceding revelations is fortunately repeated in RIE; but the content of the manuscript of vol. III remains unknown.



(4) Symmetric frame with both contiguous and intertwined letters.

RIE 303 = JE 2221 (p. 410 and pl. 191, top).

Here again, LeMAA's photograph (pl. XVI, center) is much clearer than the almost twice larger RIE's illustration and is reproduced on the right. - For the tandem's characteristic handling of the measurements of the item, cf. above, pp. 18-21. - The analysis of the frame is most interesting. The tandem sees in it "le corps d'un animal composite, à corps d'oiseau mais à tête et pattes de félin." This recital is copied from AnCFY (p. 181), with the earthshaking substitution of "félin" to "lion" (p. 410) without any cross-reference to p. 41, where F. Anfray speaks of "un être composite dont le corps suggère un oiseau à la queue éployée mais dont la tête et les pattes imitent celles d'un fauve." First of all, the reader notes the retrograde description from an individual to the family of the Felidae to the generic of wild beast! It is very unfortunate that no released photograph allows the reader to detect the head, paws or especially the hind legs of the beast involved. Furthermore, the setup of the upper four letters composing the first name contrasts with the lower two letters, smaller in size, of the second name. The frame does not seem to have any special value except to provide a border to the whole group of letters, and the three additions to the top seem to merely counterbalance the trapezoidal lower section. Finally, always most anxious to posit gratuitous pronunciamentos, the tandem speaks of "les animaux sacrés ou mythiques" (cf. above, pp. 17 and 22) = "ces animaux symbolisent évidemment des divinités du panthéon sudarabique" (F. Anfray, p. 41). Again, it is a pity that neither Anfray nor the Drewes-Schneider tandem took advantage of this case to refer to A. Grohmann's Göttersymbole..., which is listed in their bibliography (p. 533, 14th entry) and told their reader that the author's "Zusammenfassung" (p. 76) was unable to give the name of a single South-Arabian deity having the "eagle" as a symbolic animal ... and the three of them do not even speak of an eagle, but of a bird only!!!

RIE 303

The composite name may be read as follows (cf. already DrScDEE, pp. 95-96 and repeated by RIE): wʿrn, four contiguous letters forming an upside-down tau cross in the upper section, and

ḫywt, a calligram in the lower three-fourths topped by a contiguous w.

Waʿrân Ḫaywat.

(5) Frameless, symmetric calligram.

(a) Ja 3269 = JE 2193 = RIE 301 (p. 409 and pl. 187, top, extreme right).

Ja 3269

The Drewes-Schneider tandem sees "deux fois le mot ḫyw." This reading is not based on the calligram, but exclusively on the coauthors' fascination of the name ḫyw, as demonstrated below. As the root ḫw/yḫ does not exist, the lower section belong to s of which the upper part is the whole central section. Finally, there is no reason to read two names as the name read below accounts for all existing lines; therefore, the reading of either y or of two w's is unjustified; the wideness of the central tracing was intended to counterbalance the combined height of the two extreme forks. - The reading of the calligram remains unchanged regardless of the position, horizontal or vertical, of the stamp. - The tracing is reproduced on the right. The name may be read as follows: ḫwṣ Ḫawaṣ.

The following note is added because it is another stamp where the Drewes-Schneider tandem reads their favorite ḫyw.

Note on JE 1833 = RIE 293 (p. 405 and pl. 186, center); cf. also CoFH, pp. 48-49 and the photograph of 6 stamps in p. 80, top: from left to right: JE 1832, 1834, 1835, 1836, 1838, 1833 (= no. 6), which are described in pp. 48-49 as "plaques ajourées à anneau de suspension (pl. LIII, a)" which is p. 80, top. Note also that JE 1838 (= no. 5) is a variant of RIE 295 (cf. below). The 6 items are photographed on their back side, that with a circular haft.

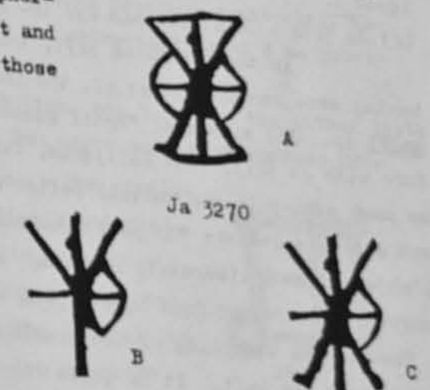
The tandem reads the name ḫyw four times in JE 1833, and their introduction literally copies a part of DrScDEE, p. 94, no. 5, but fails even to acknowledge its final remark: "On peut toutefois se demander si le mot ḫyw 'vie!' ne constitue pas ici une espèce d'invocation magique." If so, why not mention the imperative? The tandem fails to notice that the four circles are located in such a way that they perfectly parallel those of Ja 3274, viz one circle on each extremity of two straight lines so that the whole design may fit a fictitious rectangle. The tandem does neither explain nor justify the absence of a circle on each extremity of the central vertical line; there would have been six names instead of four! Why not? Then, however, the whole design would not have fitted the fictitious rectangle which is, however, discernable in each stamp. Furthermore, it is difficult not to compare this stamp RIE 293 with some ornamentations traced on red shards composed of 4 circles set, as here, in opposite positions and connected to each other by straight lines crossing each other. The difference is in the size and the location of the 5th circle: here it is a small circle in the center instead of a large one at the extremities of the straight lines; cf. F. Anfray in AE, 5 (1963), nos. 52 and 53 in pp. 111 (no JE number), 158 and 159. Moreover, if the tandem suggests the reading of ḫyw, he fails to justify or even to mention the exclusion of ḫywt. In fact, he cannot exclude ḫywt here and include it in Ja 3270. This stamp does not contain any calligram. - The tracing is reproduced above; and here again, the stippling indicates the location of the haft support.

RIE 293



(b) Ja 3270 = JE 2337 = RIE 305 (p. 411 and pl. 187, top, 2nd from the left).

DrScDEE (p. 96, no. 17) states that "On peut sans doute lire deux fois ḫyw, voir texte 5 [= JE 1833; cf. above]. Cependant le cercle du w/y n'est pas complet." Without mentioning their 1967 grand decipherment, the same Drewes-Schneider tandem repeats it and embroiders it. It is absolutely fantastic that those two coauthors never in more than a quarter of a century realized that the two horizontal lines were consistently and stubbornly eliminated. In their new (?) study, the very same Drewes-Schneider tandem suggests "soit ḫyw, soit ḫywt." Such a suggestion is appalling because the 1st reading = B discards almost the lower left half of the calligram and both its horizontal lines while the 2nd = C eliminates the same two extreme horizontal lines as well as the left curved line. The two w's must be read separately as each one forms a sharp-pointed ellipsis. Furthermore, the tandem does not justify his inclusion of the left (or right) w as the top of his y. Such an initiative remains unjustified because, as shown in tracings B and C, the right (or left) w can easily belong exclusively to y. Moreover, the reading of both y and t is not demanded by the calligram for the reason indicated in the commentary on Ja 3269 (cf. above, p. 32), viz all the lines of the calligram are accounted for by my reading. - The three tracings are reproduced above: the central rectangular space between the two w's could hide the haft support, although no haft has been mentioned so far. - As in Ja 3269, the reading of the calligram remains unchanged no matter how the stamp is set, vertically or horizontally.



A

Ja 3270

B C

A: tracing of the photograph; B and C represent the Drewes-Schneider tandem's readings, respectively, of "ḫyw" and "ḫywt."

The name may be read as follows: ḫwwṣ Ḫawwâṣ.

(c) Ja 3271 = RIE, pl. 187, top, 2nd from the right, without any identification or mention anywhere else.

Ja 3271

Here again, as in Ja 3269 and 3270, the reading of the name remains the same regardless of the position, vertical or horizontal, of the stamp. - The tracing is reproduced on the right. The calligram is composed of a name repeated twice, viz

šrr Šarîr.

The division of the two names is along an imaginary line going from the extreme left (or right) of the upper š to the extreme right (or left) of the lower š.

(d) Ja 3272 = JE 2148 = RIE 297 (p. 407 and pl. 188, top right).

As the preceding 3271, this frameless calligram is composed of letters having some rounded forms; cf. the inwardly curved tail of each y. AnCFY (p. 176) speaks of "Motif géométrique," as he had done before with Ja 3273. The calligram, further, gives twice the same name with only the initial letter repeated twice. RIE points out a single letter: "On peut reconnaître un ḫ sur un côté de l'objet," thus deliberately choosing a vague expression that prevents the reader from identifying exactly the precious spot. - The central stippling of the tracing reproduced on the right represents the haft. It is quite regrettable that the photographs of Ja 3268, 3272 and 3273 show the side of the haft and, therefore, the central section of the calligram remains uncertain, hidden, as it is, by the flat base of the haft. For the form of š, cf. below, p.43, Ja 3279/1. - The tracing is reproduced on the right. - The name may be read as follows: ↓ yšfr Yašfar.

Ja 3272

Cf. the Sabaean place name šfr in Ga 20/2.

Preliminary note on pl. 187, bottom and 188, top. - These two photographs come from the unique AnCFY's p. 231, bottom, upside down and cut vertically in two sections in order to isolate the above calligram.

(e) Ja 3274 = JE 2145 = AnCFY, p. 176 and photograph in p. 231 i, reproduced in RIE, pl. 188, top, lower left corner . - My tracing may be found immediately below (next page) on the right. Here again, the reading of the calligram remains unchanged regardless of the position of the stamp. - AnCFY's summary describes the stamp as a "Marque d'identité. Motif géométrique," but the tandem does not



identify, label, or list the stamp (p. 527A). Yet, the tandem still reproduces its photograph! Each extremity of the two long sides is ornated by a small circle, as in JE 1855; compare also Ja 3267 (cf. above, pp. 32 and 28).

Ja 3274

The name may be read as follows:

bḍ Bayḍ (rather than Baḍḍ).

The reading of the letters n or ṯ, although possible (cf. both y and ṯ in Ja 3267 and 3265) is not at all necessary (cf. above, the commentary on Ja 3269 and 3270). The reading of b, however, is demanded by the absence of any hollow or geminate root composed of ṯ and ḍ. It is also possible to read ḍb Ḍabb (rather than Ḍawb), but the root bḍ is more frequently attested. Also absolutely unnecessary and farfetched would be the understanding of ḍ as providing a frame for the calligram.

Note on JE 1713; cf. CoFH, photograph in p. 69, bottom, no. 3, which the author lists among "deux plaques ajourées" as "un carré muni d'appendices crochus" (!!) (p. 44). Cf. the tracing on the right. - The similarity with Ja 3274 (cf. immediately above) is strictly material. This identity stamp is rather poorly executed; its geometrical design does not contain a monogram because neither ṯ nor ḍ can be read; their width would exceed their height. The lateral ornamentations outside the two extremities of the two opposite sides are, on the one hand, two curved lines developing along the lateral side and completely asymmetric, that on the left reminds of the curved tail of the ibex of Ja 3265 and, on the other hand, two plain circles located not in the axis of the vertical side (Ja 3274) or outside each extremity of the small sides (Ja 3267), but in the axis of the diagonal of the right angle of the frame. - The tracing is reproduced on the right.

JE 1713

(6) Symmetry of both the calligram and the inside frame which is the longitudinal center of the stamp.

Ja 3273 = JE 2144 = RIE 296 (p. 407 and pl. 187, bottom).

Cf. the note after Ja 3272 on the origin of the photograph and the position of the item.- Here again, the stippling in the center of the tracing represents the haft. The reading of the name, which is repeated four times, remains unchanged whatever the position of the stamp might be, horizontal or vertical. - The stamp is composed of three sections; viz (a) a rectangular, inside frame containing (b) a central ʿ flanked above and below by a parabolical r which the tandem understands as "deux

Ja 3273

cupules d'un h" (copied from DrScDEE, p. 95; but, the root "šhᶜ" does not exist), and (3) each long undulating side of that frame (cf. the remark above, p. 23, top) supports two š's separated from each other by the ornamental chevron < (cf. also Ja 3264 and RIE 299). The tandem notes that "les chevrons pourraient s'interpréter comme quatre m" (copied from DrScDEE, p. 95, where the verbal form is "peuvent"). The reading of m is disproved by the fact that the four extreme aslant strokes on the outside corners of the stamp do not belong to any side of the interior frame, but to the letters. DrScDEE's commentary ends as follows: "On peut donc lire quatre fois hm. Aucune interprétation." This double statement is missing in RIE. Moreover, none of the two coauthors ever refers to the central ᶜ of which a small section is still visible between the haft and the central chevron > on the left. The two short sides of the interior frame do not support any ornamental chevron ∨ most probably because any chevron would break the image of the fictitious rectangular frame (cf. above, p. 32, RIE 293). - The tracing is reproduced on the right.

The name may be read as follows: šrᶜ Šâriᶜ.

Note on JE 1836. - CoFH, pp.48-49 and photograph in p. 80, top, no. 4, shows inside a rectangular frame two vertical, parallel series of broken lines composed each of 5 chevrons V as in the right outside of Ja 3273, and where there is no possiblity of reading any letter. This parallel is strictly material because Ja 3273 presents two r's and one ᶜ.

Note on JE 1835.- Cf. CoFH, pp. 48-49 and photograph in p.80, top, no.3, which should also be referred to here because the inside of the rectangular frame contains two sets of two chevrons V each facing the other and separated from each other by a large f. No name can be read here.

The preceding geometrical design is often utilized in pairs as ornamentation in pottery; a few examples suffice here: the three undulated lines closely paralleled to each other on the bulge of the pot JE 1850 in CoFH, p. 49, photograph in p. 80, bottom, no. 3, and tracing in p. 81, upper right; and two undulated, intertwined lines below the neck of the pot JE 2946 in AnAnM, p. 80, photograph and facsimile in p. 129, fig. 2 and 3, right, respectively.

Many examples are also given by F. Anfray in AE, 5 (1963), viz.

nos. 7 in pp. 109, 144 and 145: more than 9 lines;
8 in pp. 109, 144 and 145: a single line;
26 in pp. 110, 150 and 151: three lines;
46 in pp. 111, 156 and 157: one line and one pair of lines;



pp. 112 and 162, upper right corner: a single line also below the neck of the pot.

(7) Symmetry of both the frame and the contiguous letters inside.

Ja 3275 = AnCFY, photograph in p. 233 h.

For the special case of the photographs g and h of the same p. 233, cf. above, p. 16; cf. also the note in p. 18. - The tracing is reproduced on the right; the haft support is seen across the central letter, going beyond the apex of the left letter. - An elliptical frame in the form of the well-known ethnic symbol (cf., e.g., RIE 72, 73, 76, etc.) with three letters inside; the reading of the name remains unchanged regardless of the position of the stamp.

Ja 3275

The name may be read as follows: hfs Hafass.

Another interpretation is conceivable, but hardly possible: the inside would be another ethnic symbol perpendicular to the center of the frame and flanked on both sides by either two chevrons pointing to each other or two letters, h and s.

(8) Symmetry of both the frame and the separated letters inside.

RIE 294 = JE 1837 (p. 406 and pl. 186, bottom).

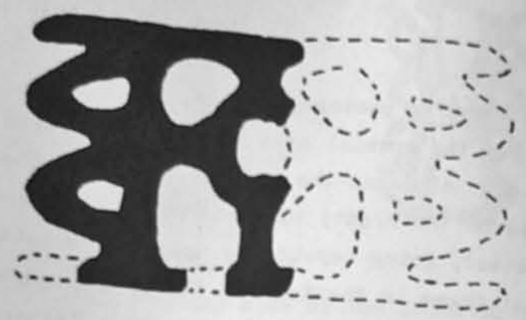
RIE's photograph, that is reproduced below in the study of Ja 3278, is the same as in DrScDEE, p. 104, fig. 7, but upside down. - As already pointed out above (p. 18), RIE's measurements are not in DrScDEE's edition, p. 95, no. 6, that refers to the parallel with Ja 3278 and states that "il faut sans doute lire deux fois gm." RIE describes the three letters, but their expert opinion does not include any reading. The reading remains identical independently from the position of the stamp.

The name may be read as follows: mqm Maqâm, as in Ja 3278; cf. immediately below.

(9) Symmetry of both the half frame and the connected letters between the two outside stiles.

Ja 3278 = JE 2882 = RIE 308 (p. 413 and pl. 192, bottom): more than the left half is preserved; the length of the fragment is given as 4.5 cm; thus, ± 7.7 x 4.3 x 0.5 cm (cf. already above, p. 18). This is the only item of which RIE gives the thickness; cf. above, l.c., and also p. 6, the remark on the JE siglum. - As he had already been forced to do in similar cases, Ja 3261 and 3279-3281, the reader has to go to the concordances (pp. 520A and 527A) to find out that the museum number of the item is JE 2882. - The tracing of the restored, approximately full-scale mark is reproduced below. - RIE's photograph of the left fragment in pl. 192, bottom, is approximately 1.49 the actual size, and both AnAnM (p. 142, fig. 4) and R. Schneider

RIE 294



Ja 3278

The photograph of RIE 294 (pl. 186, bottom) and my drawing of restored Ja 3278 are approximately full-scale.

in AE, 6 (1965), have in common the very same photograph which is 1.29 the actual size. Note that R. Schneider's pp. 94, 95, 96 are AnAnM's pp. 141, 142, 143. It is difficult for the reader without the help of a scale to figure out mentally the actual size of the mark. Since Ja 3278 is the only epigraphic appliqué known so far and, especially, since it is also known as an ordinary stamp, RIE 294, it seems fitting to represent both side by side at approximately the full-scale dimensions. - As far as the origin of Ja 3278 is concerned (cf. already above, p. 12 on Ja 3263), this item is said to have come from Matara, "du tertre C, dans la salle 4." Unfortunately, the last precision is useless to the reader because, although there is an excavated square C in the sketch of p. 54, there is no sign of "salle 4" to be found there. - RIE correctly refers to the same name in RIE 294 with a complete rectangular frame. Ja 3278 may be considered as a further adaptation of the RIE 294 solution aimed at reinforcing the horizontal axis of the stamp by adding short connecting bars between the letters in order to offset the weakness of the letters resulting from the missing lateral sides of the frame. Finally, the tandem states that "on ne voit pas de trace de poignée ou d'anneau de préhension." Unfortunately, however, they did not realize the true meaning of their remark, and the mention of an "anneau " is gratuitous because the haft is not necessarily a ring; cf. above, pp. 19, 21 and especially 24. - The restoration of the missing element in the tracing is based on RIE 294 where the two m's face opposite directions, and the reading of the name remains the same independently of the position of the item.

The appliqué may be read as follows: [m]ṛqm [Ma]qâm.

The duo of RIE 294 and Ja 3278 still remains a unique case. It would be temerarious to base any comparative study of stamp-appliqué on such limited a documentation. The absence of lateral supports readily explains the transversal bindings of Ja 3278.



Note on RIE 295 = JE 2114 (p. 406 and pl. 187, top, extreme left; cf. also above, the initial note on JE 1833 in p. 32).

This is another appliqué which RIE did not recognize as such and mistakenly understood as epigraphic: "le décor du milieu peut être interprété comme une série de n," a statement literally copied from DrScDEE, p. 95. - RIE's photograph is reproduced below; AnCFY's photograph in p. 233 f, although less precise, is nonetheless very precious because it does not show the thick,black V of RIE's photograph, that prevents the reader from seeing most of the binding between the two lateral designs. This manifestly adventitious V, which looks very much like being made by a marker, remains unexplained in RIE's so-called study. The connection between the two sections of the central unit looks like a rectangular plaque almost contiguous to the two sections. The tracing of that central unit shows the large V in hatching, the extreme left of the central longitudinal binding and, inside the V, a tiny triangular part of the same binding isolated between the two legs of V.

RIE's photograph of RIE 295.

My drawing of the right (central) unit of the applique RIE 295, showing:
1 - the thick V in hatching, and
2 - the two remaining parts of the longitudinal, central binding, not hidden by the adventitious V.

There is no "série de n," but a central unit composed of two elements which are repeated on both lateral sides. Finally, as an illustration of the confused mental attitude of the editors, the reader may compare the measurements found in two publications: viz "longueur 4 cm, largeur 2,8 cm" in RIE, but

"largeur: 4 centimètres, épaisseur: 0,5 centimètre" in AnCFY, p. 174,with reference in fn 4 to "Plusieurs objets semblables" in (without mentioning any name) LeMiRA, pp. 124, bottom left, and 125, upper left. DrScDEE (p. 95, no. 7) does not mention any measurement. The reader cannot but marvel at the terminological confusion and the fragmentary documentation of both publications.

RIE 295 has in common with Ja 3278 the following features:

a - no haft is reported;

b - the thickness is 0.5 cm;

c - both have two longitudinal bars, but in different places;

d - the longitudinal axis of the appliqué is reinforced by an intermittent line either inside (RIE 295) or outside (Ja 3278) the composite units;

e - there is only a partial frame in either the longitudinal (Ja 3278) or lateral (RIE 295) sides;

f - the missing right contains the pendant of the extreme left unit, two chevrons on either sides and the vertical frame as in its perfect parallel JE 1838 (cf. CoPH, pp. 49 and 80, top, no. 5). The two appliqués were broken at the same weak point, viz between the central and the right elements; the relation between the whole length and the existing fragment is about 1.75.

(10) Unclassifiable due to defective publications; most probably (3) (cf. above, p. 28).

Ja 3277 = JE 2339 = RIE 307 (cf. above, p. 14).

No photo, no facsimile, no description of the frame are to be found in any of the three publications involved, viz AnCFY, p. 186, DrScDEE, p. 96, no. 19, and RIE, p. 412. What an outstanding example of informative communications!!!

DrScDEE refers to his "textes 7 et 10" as parallels of the presence of n inside the frame. Those numbers are JE 2114 (= RIE 295) and 2151 (= Ja 3268) which are, respectively, a frameless non-epigraph and a rectangular frame. Therefore, the reader still does not know what to think. "On peut peut-être reconnaître deux n, séparés par deux traits verticaux" (cf. DrScDEE, l.c.) is repeated verbatim by RIE who, however, eliminates the adverb "peut-être." - Here again, the reading remains unchanged regardless of the position of the calligram.

The name could be read as follows: n(b)n (?) Naw(b)ân (?).

The reason of the question mark is the Drewes-Schneider tandem's hardly reliable reading, viz the stamp might be a copy of, or similar to, Ja 3268. This is another example of the bitter handicap caused by the fragmentary record of photographs (cf. already above, p. 28, Ja 3267).

d - The marks according to the position of the letters.

(1) The basic element of a mark is the name it is supposed to reproduce , and the frame or absence of frame will inevitably depend on the shape , calligram or not, the owner selects for his mark (cf. already above, p. 28, Ja 3267). Here follows the presentation of the marks according to the position of the letters.



| | | | |
|---|---|---|---|
| A: separated letters + frame | Ja 3264 | → | with neutral h and s |
| | 3265 | ← (or →?) | with neutral h and t |
| | 3266 | → | with neutral n, s and f |
| | 3268 | ← | with dextrograde n and neutral b |
| | 3276 | ← | with dextrograde r and neutral c |
| | RIE 294 | ⇄ | same name as Ja 3278 |
| + adjustment imposed by the circular c | 300 | → | with neutral t and s |
| | 299 | → | contrary to sinistrograde b, with neutral d and c |
| B: contiguous letters + frame outside | Ja 3267 | ← | with neutral t and q |
| | 3275 | ⇄ | |
| inside | 3273 | ⇄ | |
| C: unclassifiable | 3277 | ⇄ | either Ja 3268 or 3275 |
| D: variants: symmetric frame with | | | |
| 1: medial bars connecting separated letters + frame | 3278 | ⇄ | same name as RIE 294 |
| 2: contiguous asymmetric and intertwined letters + frame | RIE 303 | ↕ | |
| E: calligram without frame | | | |
| 1: contiguous letters | Ja 3271 | ↓ ↑ → ← | |
| | 3272 | ↓ | |
| 2: intertwined letters | 3269 | ⇅ | |
| | 3274 | ⇅ ⇄ | |
| 3: intermediary: contiguous and intertwined letters | 3270 | ⇅ | |

The designs of the 3rd column indicate the direction of each name, and the 4th column contains some remarks.

The frames of Ja 3265, 3266 and RIE 300 profile animals, viz an iber, a lion and a bull, respectively. The three beasts look to the left (cf. already above, p. 23), and, therefore, to the right in the imprinted seals. Yet, the seal can be read in either directions in Ja 3265, but is dextrograde in Ja 3266 and RIE 300. Out of the

five remaining seals of which the directions is not obvious, only Ja 3264 and 3267 must be read in the direction of the sole letter, n in each case, susceptible of indicating a direction. RIE 299, on the contrary, must be read in the direction opposite to the only letter, ʾ, susceptible of showing the direction. Ja 3268 and 3276 have each two letters facing opposite directions. Such a mixture of dextrograde and sinistrograde ductus and letters belongs to the boustrophedon period which, in Saba' probably started in the 13th-12th century B.C. (cf. JaMAR, X [1979], p. 12), an opinion which I have always held in the past. This mixture is a normal feature in those Sabaean texts from Ethiopia; cf., e.g., RIE 1, 2, 5, etc. The Drewes-Schneider tandem notes (p. 68, introduction to RIE 1) that "Les aleph sont tous orientés vers la droite." The adjective "tous" means ... two only, viz. in ll. 5 and 6! The tandem adds that "La graphie, irrégulière, est du Monumental A" which belongs, according to the tandem himself, to the "classement paléographique ... proposé par Jacqueline Pirenne" (p. 12). It is a well-known fact that the "Monumental A" of J. Pirenne contains no less than 13 (thirteen) different types or illustrations, that the inscriptions from Ethiopia do absolutely not fit any of them, and that J. Pirenne's fantasy of designs and ideas was fully rejected at length in my publication entitled La paléographie sud-arabe de J. Pirenne (Washington, 1957, 198 pages), which is not at all utilized by the Drewes-Schneider tandem - not even mentioned in the "Bibliographie" (p. 534) - undoutedly for the sake of honest objectivity.

F. Anfray was astonishingly right when he characterized the group of stamps, "des marques d'identité" as "riches d'enseignements" (p. 41, l. 16). It is very unfortunate, however, that he did not realize, no more than the Drewes-Schneider tandem, how much their publications are defective, grossly incomplete, superficial and lacking the necessary knowledge of South-Arabian palaeography. Even more pathetic and pitiful, however, is the fate of all those marks, stamps and appliqués, that will never be correctly published. Who, indeed, will ever travel to Addis Ababa to take the careful museographic description of all those precious antiquities? The same situation has already been denounced in A. de Maigret ed., The Sabaean Archaeological Complex in the Wādī Yalā...., Rome 1988. Who will ever travel to that Yemeni spot and take good photographs to replace those showing A. de Maigret's loathsome initiative described by G. Garbini as follows: "Prof. de Maigret had [.] picked them out with white chalk" (cf. JaMAR, XVII [1989], p. 5)? For C. Robin, however, de Maigret's initiative is "un pis-aller" (cf. Bulletin critique des annales islamologiques, 7 [1990], p. 177) which is not to be regretted because, as Robin puts it, "notre seul regret est de ne pas avoir lu en italien" a volume "rédigé en anglais" (p. 178)! Yet, he himself published a paper in English in D.T. Potts ed., Araby the Blest, Copenhague, 1988, pp. 168-175; for my appraisal of this paper, cf. below, pp. 127-128.



D - Three small stone seals.

The three small seals RIE 288-290 are added here because of the similarity of purpose and of presentation and also because the name of RIE 289 is seen in the stamp RIE 303. - Here again, as in Ja 3261 and 3278 (cf. above), the reader has to go to the "Concordance" at the end of the book to find out the museum siglum of each item, viz JE 59 (p. 526A) = RIE 288 (p. 519B);
3443 (p. 527A) 290 (p. 519B); and
4804 (p. 527B) 289 (p. 519B).

RIE's three photographs are of poor quality; they are not clear, neat and sharp enough to allow any tracing; e.g., the right and lower sides, the outlines of the letters and ornamentations of Ja 3279 cannot be clearly distinguished; the same must be said of the sides and the bevelling of Ja 3280, and of the different cuts of Ja 3281; therefore, the drawing of the seals cannot be made. Those photographs are reproduced here along with my facsimiles showing the marks of the imprints.

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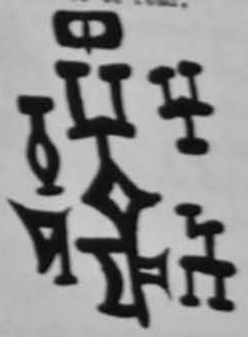
As if to keep step with the rest, the tandem fails to give the measurement of the thickness of the seal. Furthermore, the tandem correctly mentions "une inscription de deux mots" and uses the conjunction "ou" between his two suggested readings. Then, most simplistically, the two coauthors seem convinced of having accounted for the number "deux." Unfortunately, their two readings are of the same letters ... and the second name, in fact the first, still remains to be read.

RIE's photograph with three remains of a scale in the lower right corner.

Ja 3279



My facsimile of the seal as seen in the imprint.

a - The first name is composed of large, heavy letters forming a vertical calligram located in the lower 2/3 of the seal; not read by the tandem;

b - The second name contains 5 letters, smaller, completely isolated

five remaining seals of which the directions is not obvious, only Ja 3264 and 3267 must be read in the direction of the sole letter, n in each case, susceptible of indicating a direction. RIE 299, on the contrary, must be read in the direction opposite to the only letter, ʾ, susceptible of showing the direction. Ja 3268 and 3276 have each two letters facing opposite directions. Such a mixture of dextrograde and sinistrograde ductus and letters belongs to the boustrophedon period which, in Sabaʾ probably started in the 13th-12th century B.C. (cf. JaMAR, X [1979], p. 12), an opinion which I have always held in the past. This mixture is a normal feature in those Sabaean texts from Ethiopia; cf., e.g., RIE 1, 2, 5, etc. The Drewes-Schneider tandem notes (p. 68, introduction to RIE 1) that "Les aleph sont tous orientés vers la droite." The adjective "tous" means ... two only, viz. in ll. 5 and 6! The tandem adds that "La graphie, irrégulière, est du Monumental A" which belongs, according to the tandem himself, to the "classement paléographique ... proposé par Jacqueline Pirenne" (p. 12). It is a well-known fact that the "Monumental A" of J. Pirenne contains no less than 13 (thirteen) different types or illustrations, that the inscriptions from Ethiopia do absolutely not fit any of them, and that J. Pirenne's fantasy of designs and ideas was fully rejected at length in my publication entitled La paléographie sud-arabe de J. Pirenne (Washington, 1957, 198 pages), which is not at all utilized by the Drewes-Schneider tandem - not even mentioned in the "Bibliographie" (p. 534) - undoutedly for the sake of honest objectivity.

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Ja 3279

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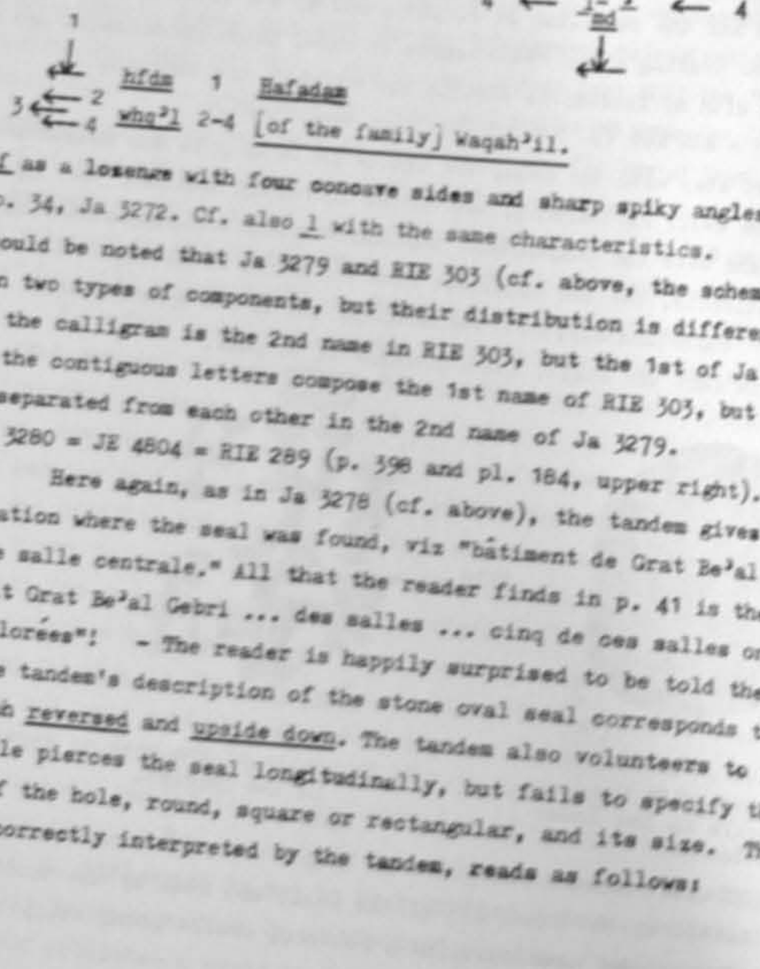
a - The first name is composed of large, heavy letters forming a vertical calligram located in the lower 2/3 of the seal; not read by the tandem;

b - The second name contains 5 letters, smaller, completely isolated

and framing the main name on three sides, the upper and both lateral sides. As h of the upper right corner of the imprint is engraved at a level markedly higher than its left counterpart, q, of which the central ellipsis lies below the central horizontal axis of the large h of the main name, I take it that it was drawn before the other. Note that that letter is q instead of the tandem "f ou q" because its sides are well rounded; by contrast, cf. the pointy f in the center. Therefore, the central line of the 2nd name, hq, as well as its 3nd line, ʾl, are sinistrograde and, consequently, the initial w also is sinistrograde as the last two letters of the calligram, dm.

The whole seal may be read as follows:




For f as a lozenge with four concave sides and sharp spiky angles, cf. already above, p. 34, Ja 3272. Cf. also l with the same characteristics.

It should be noted that Ja 3279 and RIE 303 (cf. above, the schema of p. 51) have in common two types of components, but their distribution is different, viz

a - the calligram is the 2nd name in RIE 303, but the 1st of Ja 3279, and

b - the contiguous letters compose the 1st name of RIE 303, but the letters are separated from each other in the 2nd name of Ja 3279.

2 - Ja 3280 = JE 4804 = RIE 289 (p. 398 and pl. 184, upper right).

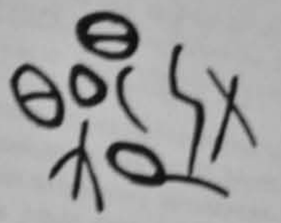
Here again, as in Ja 3278 (cf. above), the tandem gives some details on the location where the seal was found, viz "bâtiment de Grat Beʾal Gebri, à Yeha, dans une salle centrale." All that the reader finds in p. 41 is the mention of the "lieu-dit Grat Beʾal Gebri ... des salles ... cinq de ces salles ont été partiellement explorées"; - The reader is happily surprised to be told the thickness of the seal. The tandem's description of the stone oval seal corresponds to his published photograph reversed and upside down. The tandem also volunteers to inform the reader that a hole pierces the seal longitudinally, but fails to specify the form of the diagram of the hole, round, square or rectangular, and its size. The inscription, which is correctly interpreted by the tandem, reads as follows:



A

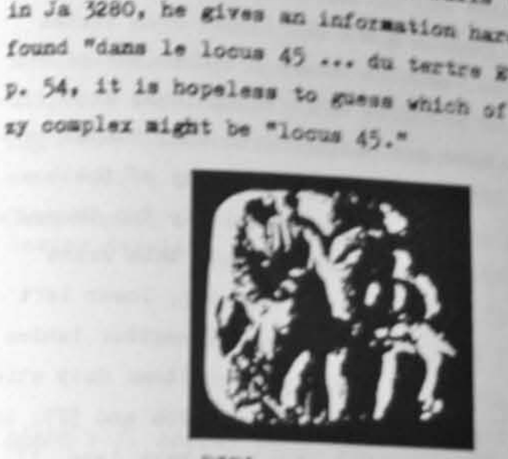
RIE's photograph of RIE 289.

Ja 3280



B

My facsimile of the imprint.




1 Waʿrān
2 Hay-
3 wa-
4 t.

3 - Ja 3281 = JE 3443 = RIE 290 (p. 399 and pl. 184, bottom).

As in Ja 3279, the tandem fails to give the measurement of the height; and as in Ja 3280, he gives an information hardly useful to the reader, viz the seal was found "dans le locus 45 ... du tertre B1, à Matara." If it is easy to find "B1" in p. 54, it is hopeless to guess which of the small squares and rectangles of that mazy complex might be "locus 45."



RIE's photograph of RIE 290.

My facsimile of the seal.

The tandem informs the reader that "l'objet est percé d'un trou." Where is it? What is its size? No detail is given. - Here again, as in the three open-worked metal stamps with animal frames, Ja 3265, 3266 and RIE 300, the animal on this seal is also looking to the left. With long legs and head, the animal could be a horse; the tail is hardly visible on the photograph. - The name seems to be composed of three letters, viz an upside-down h (cf. also Ja 3265) above the animal's rump, a pointy r on the left side of the head and above its neck and, also, what looks like a t in

in front of the animal's breast. I suggest reading the name as follows:

Garrat.

E - Apotheosis.

While progressing in the study of the Drewes-Schneider tandem's extraordinary presentation of three different kinds of short identity cards, whether in metal or stone, the reader was constantly taken up with the anxiety of finding a grand finale abreast of their work, that might crown and even enhance their exceptionally prize-worthy performance. Fortunately, the two coauthors' brilliancy and premonition had already left the reader behind; they had already foreseen his wish and provided him with an answer. But, masters of the blindman's buff, they had hoped to fool the reader by placing their crowning jewel somewhere inside the whole material. Their preeminent jewel is pl. 185 with two different seals which remain unnumbered and unmentioned in pp. 395-400 with RIE 288-290 and 401-413 with RIE 292-308. These two epigraphic shards should be RIE 291 and 292. But RIE 292 is something else, viz Ja 3264. And RIE 291 is Paribeni's seal which does not belong to the group nos. 288-290 (cf. above, p. 14, a similar case in RIE 309), and no. 291 is not even listed in p. 519B of the "Concordance des inscriptions selon le numéro d'ordre dans ce recueil" (pp. 517-521). This feature, however, is far from being exceptional as 53 (fifty-three) other numbers existing in the volume also are missing from that "Concordance;" viz 44, 45, 49, 50, 64, 66, 70, 83, 88, 107, etc. The peculiarity of the two photographs of pl. 185, however, is that they were selected exclusively for themselves since each of them represents one item only. This situation sets them aside from Ja 3271 (pl. 187, top, 2nd from the right) and 3274 (pl. 188, top, lower left corner). For these two photographs of pl. 187 and 188, the Drewes-Schneider tandem could, and probably would, invoke the excuse that they contain other items duly utilized in the book, viz Ja 3269, 3270 and JE 2114 in pl. 187, and Ja 3268 and 3272 in pl. 188. At the very best, such an excuse would be at the very most very lame. At any rate, to select two photographs representing items not even mentioned in the book can hardly be attributed to anything but an ungoverned mind.

F - General conclusions.

The following lines merely put together the main characteristics of the various publications since 1955 on the small Sabaean marks discovered in Ethiopia. The very skeletonic museographic description inaugurated by J. Leclant - A. Miquel in AE, 3 (1959), pp. 107-130, was mimicked by all subsequent reports and more recently



by RIE (1991), and the lack of photograph complemented by the absence of even an approximative sketch, as it is the case of Ja 3277 (cf. above), testifies to the pitiful, careless amateurism of the writers who dealt with those precious relics of the past.

It is also obvious that RIE's work practically rehashes, and even sometimes shortens, previous reports without adding any substantial inside or any new systematic analysis. The only exception is a most shameful novelty, an unheard-of gross fallacy trampling under foot the very essence of palaeography which, by nature, demands full respect to every part of every letter. And to make very sure that the reader would unmistakably know that it is the Drewes-Schneider tandem's finest scientific achievement rightly honored by the "Académie des Inscriptions et Belles-Lettres," and not at all some cheap, capricious or witty expedient, these coauthors offer him not one isolated example, but four (Ja 3261, 3262, 3263 and 3270) where they voluntarily, systematically eliminate parts of letters to satisfy their embryonic knowledge of South-Arabian letters, which is further illustrated by Ja 3266, 3267, 3271, 3272, 3274 and 3275.

The preceding summary of the most important characteristics of the Drewes-Schneider tandem's work on the South-Arabian stamps, marks and seals from Ethiopia inevitably leads to the ultimate fate it so rightfully deserves, viz that RIE be withdrawn from any further sale and either be sent to the power hammer or be shredded.

(Final note. - A few cross-references may have been inadvertently bypassed. The following detailed indexes will largely compensate the reader for those omissions.)

***

Indexes

Abbreviations

AnAnM : F. Anfray et G. Annequin, in AE, 6 (1965), pp. 49-86 and 93-144.

AnCFY : F. Anfray, in AE, 5 (1963), pp. 171-236.

CoFH : H. de Contenson, in AE, 5 (1963), pp. 41-87.

DrScDEE : A.J. Drewes et R. Schneider, in AE, 7 (1967), pp. 89-106.

JaMAR : A. Jamme, Miscellanées d'ancient arabe, Washington.

LeMAA : J. Leclant, in BSAC, 16 (1962), pp. 289-304 and 20 plates.

LeMiRA : J. Leclant et A. Miquel, in AE, 3 (1959), pp. 107-130.

RIE : E. Bernand, A.J. Drewes et R. Schneider, Recueil des inscriptions de l'Éthiopie des périodes pré-axoumite et axoumite (Académie des Inscriptions et Belles Lettres), Paris, 1991, 2 vol.

antiquity list

AnAnM, pp. 80 and 129, fig. 2 and 3, right = JE 2946
AnCFY, p. 231, p. 34
i = Ja 3274
p. 233 g, pp. 16, 21, 25
h = Ja 3275
F. Anfray, in AE, 5 (1963), no. 52 in pp. 111, 158 and 159, p. 33
53 in pp. 111, 158 and 159, p. 33
CoFH, pp. 44 and 69, bottom, no. 3 = JE 1713
pp. 48-49 and 80, top, no. 1 = JE 1832
2 1834
3 1835
4 1836
5 1838
6 1833
bottom, no. 3 and p. 81, upper right = JE 1850
DrScDEE, pp. 94 and 104, fig. 5 = Ja 3267
Ja 881A = RÉS 3519 = 5028, p. 30
3261 2630 = RIE 43, pp. 3, 6, 7, 10, 13-15, 37, 43, 47
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3267 1839 = DrScDEE, pp. 94 and 104, fig. 5, pp. 3, 19, 25, 26, 28, 35, 40-42, 47, 50
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3275 = AnCFY, p. 233 h, pp. 3, 15, 16, 19, 21, 25, 29, 37, 41, 47
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3277 2339 307, pp. 3, 14, 15, 18, 19, 21, 24, 26, 40, 41, 46, 47
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123 = Ja 3264
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JE 2946 = AnAnM, pp. 80 and 129, fig. 2 and 3, right, p. 36

RIE's concordance (pp. 526B-527A) does not list the nine following JE numbers: 1713, 1834, 1838, 1839, 1850, 2145, 2340, 2353 and 2946.

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3570, p. 26
5028 = Ja 881A

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185, pp. 4, 46
187, top, 2nd from the right = Ja 3271
188, top, lower left corner = Ja 3274
192, p. 14

Personal names of the marks

ʾʿdd RIE 299
bḏ Ja 3274
hsn Ja 3264
hfdm whqʾl Ja 3279
hfs Ja 3275
wʿrn ḥywt Ja 3280, RIE 303
whqʾl, cf. hfdm
ḥwwz Ja 3270
ḥwṣ Ja 3269

ḥywm ġq Ja 3262
ḥywt, cf. wʿrn
(ḥnʾt, cf. tʾnḥ)
ḥrt Ja 3281
yšfr Ja 3272
klbm Ja 3261
mṭs RIE 300
mqm (Ja 3278), RIE 294
n(b)n Ja 3277

srfh Ja 3266
ġylm Ja 3263
ġq, cf. ḥywm
qtn Ja 3267
r(b)n Ja 3268
rnʿ Ja 3276
šrr Ja 3271
šrʿ Ja 3273
tʾnḥ Ja 3265 (cf. also ḥnʾt)

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(May 1994)

## II - The inscribed Luristan (?) bronze sword:
## Danish national museum 14646 a and b,
## and Ja 3232.

### 1 - Introduction.

The bronze sword with a short inscription and a calligram on the hilt, no. 14646 of the Nationalmuseet, "allegedly from Shir-i-Shiqat" (p. 147) "in Luristan, western Iran" (p. 136), was published by H. Lassen - V.F. Buchwald - W.W. Müller (respectively, for "the archaeological conclusions," "the scientific" and "the philological" questions, p. 136A, under the asterisk) in AfO, 35 (1988) 1990, pp. 132-152: "A Bronze Sword from Luristan with a Proto-Arabic Inscription."

After thanking the persons in authority at the museum, as wont wills it, "my thanks also go," adds Lassen, "to Dr. Daniel Potts, who read and commented on an earlier draft of this presentation" (cf. fn 1, p. 136A). Potts' comment must have been both fascinating and wonderful to inspire such special a mention. It is, therefore, quite astonishing not to find anywhere in the paper even a mere hint of that exceptional contribution to science! The reference to Potts' 1978 and 1984 papers (cf. fn 27, 30, 31) cannot evidently be taken into consideration for the question at hand. Instead of portraying Lassen as unfair to his alleged enlightener, it might be better to ascribe his compliment to the rather lame understanding of it as a mere lapsus calami brought about by the frame of mind he was in when thinking and thanking the authorities of the museum.

In the last fn of the paper, introduced by another asterisk, Lassen informs the reader that "this paper has been presented at the II. Anatolian Iron Ages [sic!] Symposium, 4-8 May 1987, Izmir, Turkey" (p. 152B). It is a pity that such a long round trip of about 4,400 km (as the crow flies) for the sole purpose of informing the scientific world of the combined efforts of no less than nine formally named professionals contributing to the seemingly exhaustive study of an ancient inscribed bronze sword was actually used to entertain an international assembly with the decipherment of a text read in the wrong direction, the misreading of two letters out of six, and the misundertanding of the calligram as a "whip"! (cf. below); and this triple failure is like a three-peak island in the midst of an uncharted ocean with a confusing, incomplete description of the designs ornating the sword.

A color diapositive of the hilt was sent to me by the Department of the Near Eastern and Classical Antiquities of the museum. The absence of scale on that diapositive - the same absence is also noted in fig. 4 (p. 139), the only enlarged photograph printed in the paper -, and the unavailability of closeup photographs of



all engraved designs make it shamefully impossible to even attempt a definitive publication of the sword.

Ten close-ups are needed to complete the study of the sword, viz one of each of the five ornaments listed below, viz A2, B2, B3, C2 and D2 , one of the upper central section of A1 = C1, and one of each of the four lateral sides of the pommel in order to identify the two pairs of lateral designs. My letter of January 9, 1991, requested the museum "to send me the closeup color diapositive of the text and the lines framing it" (underlining in the original). The museum's answer of February 2, 1991, informed me that "we send you a color diapositive 1:1 of the inscription. From this you can make slides to enlarge the inscription." The museum slide, 8.8 x 11.8 cm, is manifestly not a close-up and, therefore, it is not what was requested by my letter. The substitution of a large ordinary slide for a close-up which was unjustly imposed upon the petitioner without his knowledge or approval, is neither explained nor justified in the museum letter. How is it possible for a national museum not to be able in 1991 to deliver a close-up, whatever the circumstances may be? Furthermore, as stated above, the slide does not include a scale assuring the accurate measurements of both the hilt and the inscription. The scale "1:1 of the inscription" stated in the museum letter indeed is questionable. As stated below, the total length of the text and the bars is 33 mm in fig. 5 (p. 140A); but it is 31 mm on the slide. Finally, the dimensions of the diapositive made it necessary to order a close-up from it, thus creating additional expenses caused by the museum's unauthorized initiative. It must be emphasized that the practical impossibility of getting the ten needed close-ups comes exclusively from the fact that the Nationalmuseet's invoice sent on behalf of its "fotograf," Lennart Larsen, amounts to DKr 575.00 = US$ 110.57 (at the time), including the Danish Bank Commission of ...US$ 8.85! Here follow the copies of the two relevant documents:

1 - My invoice on the Riggs National Bank, Washington, DC, no. ID-015-502516 Universal, February 25, 1991, is as follows:

"

| | | |
|---|---|---|
| Riggs Bank Draft | | US$ 110.57 |
| Danish Krones 575.00 @ $.1769 = $101.72 | | |
| Danish bank commission: $8.85 | | |
| To: Lennart Larsen | Total: US$ | " |

2 - Larsen's invoice dated of "4.2.91 is reproduced below:

Note: the printed part of the invoice is 22.2 cm wide; 3 cm of the blank vertical center were removed so that the photostat might nicely fit the page.

"

LENNART LARSEN
fotograf
Nationalmuseet/Frederiksholm Kanal 12
1220 København K
Telefon 33134411/Giro 2093278

4.2.91

Prof. A. Jamme
The Catholic Universitet
Washington.

| | | | |
|---|---|---|---|
| 20409 | 1 farveopt. 9x12 incl. eksp. | DKR. | 575.00 |

"

In spite of such an extravagant price tag, which belongs to the highway robbery business (cf. my letter of February 26, 1991), I still did not have any color close-up and the accurate measurements. Therefore, the total cost of the ten needed close-ups would then have exceeded US$ 1,400.00!!! Who could even then have afforded such outrageous an expense for ten closeup photographs?

2 - Inventory of the designs.

In order to facilitate the reference to the designs engraved on the sword, as the descriptions of the edition are both incomplete and confusing (cf. below), I have tallied them on the basis of the four drawings of the sword in fig. 2 (p. 138), which are labeled A-D from left to right, and the subdivisions, 1-2/3, run from top to bottom; thus:

fig. A1: the upper central section of the pommel; no photo and no facsimile are found in the edition; = C1;

A2: a zigzag and crisscross complex in the upperpart of one side of the blade; cf. also fig. 1b (poor photo) and 6 (enlargement; cf. below);

B1: the lateral (on the right-hand side of A1) extremity of a geometrical design spread over A1 = C1; parallelism demands B1 to be identical with D1; no photo and no facsimile in the edition; but an erroneous scribbling in fig. 2;



fig. B2: an inscription in the center of the hilt, same lateral side as B1; cf. also fig. 4 (photo), 5, top (small photo) and 12 (sketch of the tool marks);

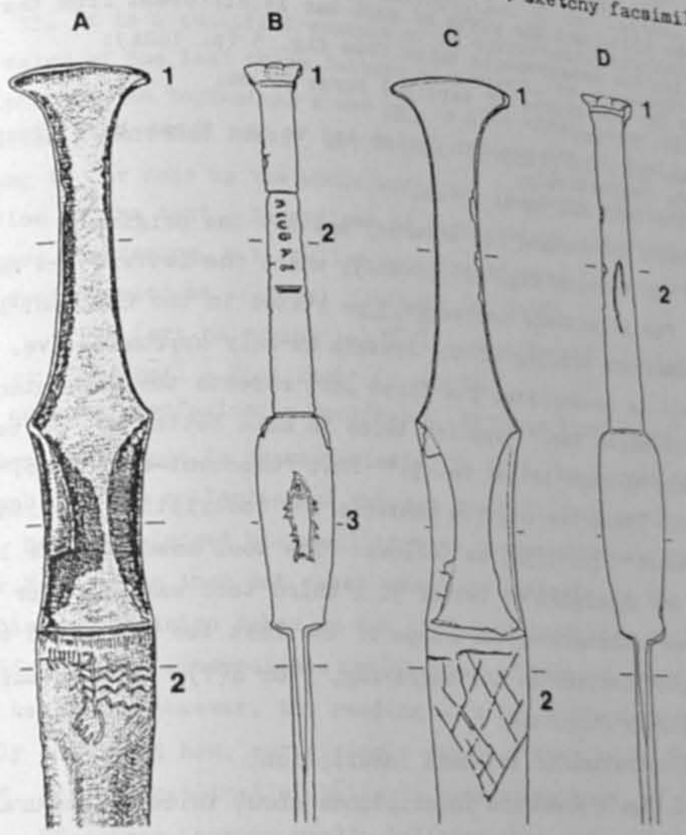
B3: a fish looking upward to the inscription, in the center of the cross guard; same lateral side as B1; cf. also fig. 5, bottom (photo);

C1 = A1;

C2: a checkered design in the upperpart of the wide side of the blade, opposite to A2; sketchy facsimile in fig. 2; cf. also fig. 1a (unclear photo);

D1 = B1 (cf. above); no photo and no facsimile in the edition;

D2= a very simple planoconvex figure in the center of the hilt; opposite to the text of B2; no photo in the edition; sketchy facsimile in fig. 2.




Upper section of the drawing of the sword (cf. fig. 2 of p. 138) made by Poul Christensen (cf. fn 1 of p. 136A). - The alphanumerical identifications are mine.

The following notes are focused on the main text of which the edition is unacceptable. The study of all designs, of which the published descriptions are as unacceptable as the edition of both the main inscription and the calligram, is needed as a complement of the main text and its chronology.

3 - Description of the main inscription.

The main text is described as "an inscription (Fig. 4) on one of the hilt flanges consisting of six signs bounded by vertical lines on each side" (p. 136B). This statement is repeated in p. 145B, where the following details are added: "the deepest penetration into the bronze [is] 0.6 mm;" the letter height is "3-4 mm;" and "the entire length between the vertical lines is 31 mm." The reader is still not told how many vertical bars there are; how they are distributed in the two places; and how long and wide they are. He has to consult the photo of fig. 4 (p. 139, bottom) to find out not only that there are three bars on the side of the cross guard and one on that of the pommel, but also that, contrary to fig. B2, the isolated bar does not reach either edges of the hilt, and the width of each bar is different from that of the two others. Here follow the measurements taken from fig. 5 (p. 140A):

distance between the two groups of vertical bars: 29 mm;
length of the text: 23 mm;
distance from the text to the one-bar design and to the three-bars group: 2 and 4 mm, respectively;
total length of the text and bars: 33 mm.

All these measurements should be checked for accuracy against the original.

The purpose of the sketchy fig. 12 (p.146A), where the letters are numbered from right to left, is "to illustrate toolmarks," as stated in the caption. A simple look at fig. 4 shows that the tracing of the letters is only approximative. Furthermore, the caption itself is incomplete. Its first part repeats the commentary of pp. 145B-146A on "a crescent-edged tool" "applied twice to make letter no. 3, twice to make letter no. 4 and once to make letter no. 5." That "crescent-edged tool" is presumably a U-shaped gouge. Then, the caption comments the description mentioning "at least three different tools" (p. 145B) as follows: "The tool used to make letter 2 was also applied to form the upperpart of letter 3. A third tool was used for letter 6." In spite of all this new information, the shape of the last two mentioned tools as well as the number of tools involved in the engraving, 3 or 4(?), still remain unknown; and letter no. 1 is simply forgotten.

4 - Illustration of the main inscription.

The photograph of fig. 4 shows the inscriptions about twice its natural size; but this detail is not pointed out. It is a pity that the photograph of the text was not given at least the size of one of the five photos on photomicrography (fig.7-11 in p. 142, 143 and 145)! There is ample space in p. 139 where fig. 3 is unjustified (the names could have been advantageously added to fig. 15 of p. 151B), and especially in the blank left part of p. 137 for an enlargement of fig. 4 which would have made the palaeographic study of the letters as easy as certain. The exceptional



importance of the text assuredly deserves at least such a treatment. But, in contrast with the meagre, niggardly illustration of the inscription, an extravagant illustrative profligacy takes place with regard to ... the "geometric decoration" of fig. A2, which is enlarged 4.5 times in fig. 6 (p. 140B):

5 - Decipherment of the main inscription = text =.

a - The most basic question, viz the direction of the line, was apparently never raised or doubted. Even from the very start the sinistrograde direction was taken for granted; cf. the end of fn 17 (p. 149B), where it is stated: "Professor Frede Løkkegaard ... pointing towards the meaning of the last three letters being the Arabic noun h-n-'," and also the numerotation of the letters, from right to left, in fig. 12. It is a qualified example of an aprioristic simplemindedness to determine the value of the last three letters of a short, very ancient name (1) without even deciphering the beginning, since hn' is also known to be an adjective, a verb and more often a personal name; (2) without actually knowing whether those three letters belong to or make up the whole word; and (3) more basically, without securing the direction of the text. Regardless of such obvious objections, but seemingly bent only on base flattering, W.W. Müller eagerly endorsed Løkkegaard's reading and completed the decipherment as follows: "lḥfnn'" (p. 149B)

"For (or: belonging to) Ḥafī - good luck!" (p. 149B; repeated in p. 150A)

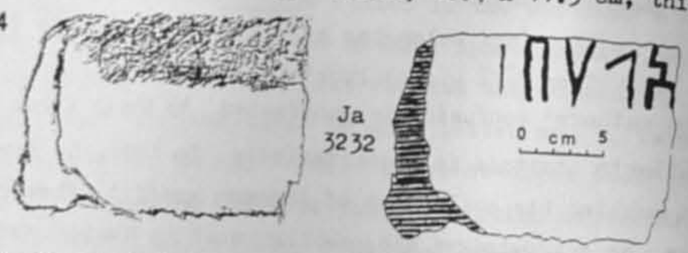
or "For Ḥafī - good luck" (p. 152B).

The authors' confusion is manifested by their first reading, viz "lh(?)gnn'" (p.149B): Müller's attitude is characteristic. In 1976, he turned down P. Kortler's invitation to publish his collection of Sabaean graffiti from northern Yemen. Several years later, he acknowledged his unwillingness to decipher some Sabaean graffiti from a cave near Mârib. Is that not clear enough to indicate to the scientific community that, by his own decision taken in two different instances including compatriotes, Müller manifested his incompetence and/or scorn of Sabaean graffiti or cursive lettering? In the meantime, however, the reading of a few books on North-Arabic inscriptions seemingly convinced him, surprisingly enough, "that he is the long-awaited messiah in NA." For the rebuttal of Müller's blundering hoax, cf. JaMAR, XIV ( 1985), pp. 144-175. Of course, nobody really believed that such a detailed confutation would open Müller's eyes or repress his pretentions. His own ego and his inbred eagerness of flattering persons capable of fostering his future evidently prevented him from turning down the Copenhague invitation; instead, he happily jumped into the wagen, all too happy to burn incense to Løkkegaard by endorsing his reading; and he produced a piece of literature that will remain for ever an outstanding, unequalled example of

inordinate, sheepish lust of popularity.

It is a matter of fact that the direction of the line is dextrograde, viz the opposite of that of the edition, and the whole published study and commentary are thus worthless. Indeed, letters nos. 1, 2 and 6, respectively ʾ, r and l, the only three letters the forms of which indicate the direction of the line, are dextrograde while the forms of the three remaining letters, h, f and s, can equally be read in either directions. Thus, not even a single letter even suggests the sinistrograde direction blindly taken by Løkkegaard and sheepishly gobbled up by Müller. Letters nos. 3 and 5 are the very well-known old types of shaftless h and s, respectively, cf., e.g., above, p. 37: Ja 3275, the following graffiti from Wâdî al-Jûbah: Ja 3116 c/1, 3121 d, 3155 ai 2, 4, 5, and Ja 3127 d, 1, 3143 a, respectively; cf. already Ja 2636 in JaMAR, IV (1973), p. 49 and pl. 8: sbh/ IVΠΛ ; p. 53 and pl. 9: Ja 2657e: /rbh, and 2658: gnh, and p. 69 and pl. 13: Ja 2735e: šhn. Beside these cursive texts illustrating the strong presence of those two old forms side by side with the many varieties of the more recent type which include a short vertical bar to support the primitive figure, Ja 3232 offers the old-time character among forms of the pure monumental style.

Ja 3232: a limestone block recut on the upper and right sides; 16.4 x 11.5 cm; thickness: 9.6 cm; text: 10.4 x 3.8 (ʾ) to 2.9 (word divider) cm. - YM 991.




ʾlhb/ ʾAlhab.

Cf. Ja 2140/5 and Safaitic CIS 4502. A. Avanzini's worthless Glossaire des inscriptions de l'Arabie du Sud 1950-1973, Florence, II (1980), p. 31A, does not refer to RÉS 3087/21 of which J. Halévy's copy has ʾlhb (cf. JA, 19 [1872], p. 252), corrected by RÉS (VI, p. 12) to "ʾl[w]hb" without giving any justification. Thus, Avanzini rejects RÉS's correction while refusing, here again, without any justification, to register Halévy's copy. Cf. also, e.g., the masculine personal names lhbn in Thamudic Hu 137, and lhhb in Safaitic ClAS 75 (cf. JaMAR, XIII [1983], p. 39). The preceding paragraph clearly shows P. Løkkegaard's presumptuous temerity of even relating the text to "possibly the Thamudic inscriptions" (cf. fn 17, p. 149B) and W. Müller's foolhardy, base flattery; but it also shows how accurate was E. Nielsen's flair of connecting the inscription with "possibly [the] Minaean-Sabaean sphere" (cf. l.c., p. 149A). In the same footnote, Lassen states to be "grateful" only to Nielsen for his correct suggestion which he discards, but "most thankful too" to Løkkergaard for all his errors; thus, declaring for the loser.



The preposterous reading of nos. 2 and 5 as "n" (cf. above, "hnʾ") and "h" (cf. "hf"), respectively, originates from palaeographic ignorance. Of unusual value is the brand new form of ʾ, no. 1, where the three short bars making two right angles in the upperpart of the ordinary form are replaced by a long, aslant, slightly concave bar starting above the upper extremity of the right vertical staff of the bottomless rectangle and protruding the left vertical staff of the same rectangle below the upper left angle. The accurate tracing of the letter ʾ is difficult to see in fig.4, where the upperpart of the letter looks like a small "r" which becomes an acute accent, "´" in fig. 5 which, in turn, becomes an unintelligible ⌊ in fig. 12 (p. 146A)! Finally, the upside-down position of l indicates once more the great freedom enjoyed by the early Arabs in their writing without in the least jeopardizing the latter's reading.

b - The philological commentary could easily be dismissed out of hand because of the wrong direction of the reading. It gives, however, the reader some precious examples of narrow-minded thoughtlessness which should not be passed over in silence.

1 - Ar hanâʾ means "happiness, bliss," which is not at all synonymous of "good luck," mentioned by Müller (p. 150A) without any reference (!), it would still be a gross anachronism to impose such late translation to a text engraved about 15-16 centuries before Islam, an anachronism that those pan-classical Arabic fanatics cannot apparently even suspect.

2 - Müller does not - of course - justify his choice of "hnʾ" as a noun instead of a verb or an adjective or a personal name.

3 - Müller does not - of course - justify the dissection of his "lhf" into two elements, viz l + ḥf.

4 - Müller's text, "lḥfhn'," would more readily read "lḥf hnʾ Lâḥif has rejoiced," understood: [in it] = in having the sword. For lḥf as a personal name, cf., e.g., Safaitic ClAS 723a; for hnʾ, cf. Arabic haniʾa "to rejoice" (cf. E.W. Lane, Lexicon, p. 2902B). With much less probability (for it would impose an unnecessary dissection of the text), one could read l + ḥf hnʾ. The meaning of "For" for "l" is unjustified because such a meaning would imply the sword to be a gift to a third person; yet, there is no indication whatsoever that such might have been the case. The "l" should be the lâm auctoris or possessionis, and hnʾ would be either a verb (cf. above), a noun "happiness" by which the owner would express his joy of having such a sword, or with no probability (cf. above, for the reason) h + nʾ, with nʾ as the present par-

ticiple of *n³y*, cf. Arabic *na³â* "to be far, distant," and *nâ³in* "far, far away." This interpretation would indicate the author as a foreigner from a faraway country.

5 - Müller's comment on his "*ḫf*" shows the well-publicized peculiarities of his argumenting period, viz his staunch anachronic pan-classical Arabic fanaticism, his defiant intrusion upon fields outside his competence, and his distortion of the written word. (a) He first states that his "*ḫf*" is "a name which in exactly the same writing is attested in the Safaitic inscriptions" (p. 150A). The mention of the Safaitic comes from the unique publication he consulted. The reader may be interested in knowing that the name *ḫf* also exists in Thamudic HaLi 407, as already noted in 1973 (cf. JaMAR, IV, p. 136, note 74; from the root *ḫff*), twenty-one years ago!

(b) The only book utilized by Müller is mentioned in fn 19 (p.150A), viz WH "nos. 2442, 2983, 3010 and p. 569, where the name is read *Ḫaff which obviously does not exist in Arabic." The phrasaeology of the dependent phrase introduced by "where" normally means that the vocalization involved is that of the two coauthors in their book. But, this is untrue: the vocalization "*Ḫaff" is not utilized once in the translations, but is simply quoted from G. Ryckmans, Les noms propres sud-sémitiques, p. 57A. The last relative sentence, moreover, means, in Müller's mind, that any vocalization unattested in classical Arabic cannot possibly exist in pre-Islamic time. This is a gross anachronistic fallacy. Finally, the Safaitic personal name *ḫf Ḫaff* is also found in Wi 211 (instead of "*ḫš*" of the edition) and in Damas 5721 = 13093 (not "109093" as in G.L. Harding, An Index and Concordance..., Toronto, 1971, p. 193, after al-Abbath, 22 [1969], p. 10, no. 46).

(c) Müller's pan-classical fanaticism is so blinding and overpowering that he himself gives what he holds to be the perfect justification of his vocalization, viz "A certain al-*Ḫāfi* bin Quḍā*ᶜa is to be found in Arabic genealogical works" (p. 150A) with a single reference to W. Caskel's Ǧamharat an-nasab. Müller's narrowmindedness is so sillily simplistic that for him it suffices to find any kind of Arabic so-called parallel to declare it ex cathedra to be the ultimate proof of his vocalization. The etymology of *ḫf* from the root *ḫfy* would be the very last possibility to seek if there were no *ḫff* or *ḫyf* roots. But, both exist and even the second is well-known in Yemeni onomastics, viz two villages called each al-*Ḫayfah* (cf. R.T.O. Wilson, The Investigation, Collection and Evaluation of Geographical Material in Yemeni Texts for the Mapping of Historical North-West Yemen, Ph.D. thesis, Cambridge, 1980, p. 252 = Gazetteer of Historical North-West Yemen in the Islamic Period to



1650, Hildesheim, 1989, p. 151), and the "Banû *Ḫayf min Wâdiᶜat" (cf. D.H. Müller, "Index historicus," p. 141A; and L. Forrer, Südarabien, Nendeln, 1966, p. 119). This tribe is located by E. Glaser east of *Ṣaᶜadat (cf. D.H. Müller und N. Rhodokanakis, Eduard Glasers Reise nach Mârib, Vienne, 1913, map no. 2). And to witness the fact that W.W. Müller does not use any of the four above-listed publications that do not have to be exhumed from some cavernous alley in the suq of a faraway town, remains a shock because, although he always carefully avoids referring to any author disputing him, a policy adopted years ago by the laudemus invicem club members, Müller always does his very best to project of himself the image of an all-knowing robot.

Another recent example of Müller's pan-classical Arabic fanaticism is his publication of *msᶜd* of Dât-Radâᶜ 1a; cf. A. Gingrich und J. Heiss, Beiträge zur Ethnographie der Provinz *Ṣaᶜda (Nordjemen), Vienna, 1986, p. 161 and photograph in pl. 1 of 177, top.

(a) Müller's fanaticism is so powerfully blinding that, for him, there is no South-Arabian graffiti in northern Yemen; they all belong to what he foolishly calls "*Qaḥṭānisch;" cf. l.c., p. 162, note 12a, where he refers to his contribution in W. Fischer, Grundriss der arabischen Philologie; I, Sprachwissenschaft, Wiesbaden, 1982, pp. 30-36 (instead of "34ff." in p. 175), and especially pp. 32-35. This section is a mass of aberrant, insane interpretations. Two examples suffice here. The section begins with his endorsement (of course, since he is a member of the laudemus invicem club) of the reading of JsaL 71/4-8 by A.F.L. Beeston et al. (= P.V. Winnett, J. Ryckmans and M. al-Ghul) in PSAS, 3 (1973), pp. 69-72. The reading of those five short lines by Beeston and his stooges contains 12 (twelve) errors!!! Then, dealing with Liḥyanite nfs, Müller does not know what he is talking about when he writes that nfs "im Lihyanischen nur in der Bedeutung 'Seele' belegt ist" (pp. 32-33). The reader may find my rejection of Müller's North-Arabic and especially his Safaitic aberrations in JaMAR, XIV (1985), pp. 145-175.

(b) The name *msᶜd* of the country of *Ṣaᶜadat becomes thus another casualty of Müller's pan-classical Arabic fanaticism: he refers it only to three Sabaean texts from Qaryat al-Fâw which, in his jargon, is declared to be "*Qaḥṭānisch." It is easily understandable why Müller's fanaticism forced him to dishonestly eliminate *msᶜd* of Ja 2849a from J. Balaq Janûb, near Mârib. This inscription is considerably more ancient (at least 10 centuries) than all his so-called Arabic texts, and was published in 1976!!!

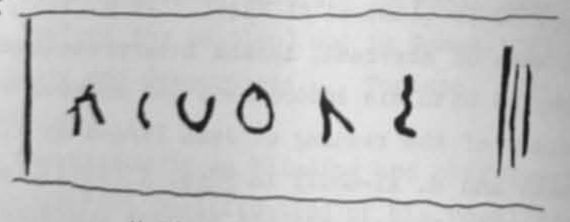
(c) Of course, the Arabic name Mas$^{c}$ûd is very well-known; cf., e.g., W. Caskel, Ǧamharat ..., pp. 401B-403A, with an impressive list of 80 references ... and of course - what else could he do? - Müller declares it to be the only possible vocalization of Sabaean ms$^{c}$d. Nonetheless, one finds "Messaad" and "Messad" in Vocabulaire ... des noms des indigènes ... de l'Algérie, Alger, 1885, p. 241A, or 1891, p. 285. What a shame that those Arabs could not have consulted with Pythonic Müller and did not know their native language well enough to avoid his disapproval!

(d) Müller's blindness also prevented him from being aware of his Janus-faced attitude. ms$^{c}$d is vocalized "Mas'ad" by A.R. al-Ansary, Qaryat al-Fau..., Riyadh, 1982, p. 143, no. 5. Müller's inborn reflex of flattering people whom he may eventually need to foster his future rang the alarm bell. So, for al-Ansary's vocalization of "Mas$^{c}$ad" and of "$^{\flat}$Arš," Müller put on his well-patted kid gloves and enchantingly states that these two forms "in Nordarabischen anscheinend nicht nachzuweisen sind" (p. 161). But, when he feels fully backed by the laudemus invicem club members, his vitriolic fanaticism fiercely bursts out, viz he refers to my vocalization of the name as "Musa$^{c}$ad" which is maintained here, as "allerdings mit der völlig unarabischen Vokalisierung Musa$^{c}$ad" (p. 161). For Müller and al-Ansary's "ms'd," cf. below, p. 126.

c - The dextrograde text of the Nationalmuseet 14646 a reads as follows:

→ /$^{\flat}$rhf sl/// Sayl has sharpened [it].

My tracing from my diapositive (without a scale) is reproduced on the right. As in many ancient texts, the complement of the verb is the object bearing the inscription. - $^{\flat}$rhf, cf. Arabic rahufa "it (a sword or a thing [of any kind]) was, or became thin, and slender" = rahafa, which has the same meaning as the 4th form $^{\flat}$arhafa "to make it (sword or a thing [of any kind]) thin; sharp, or pointed; sharp, or keen" (cf. E.W. Lane, l.c., p. 1170A-B). Note the 4th verbal form in $^{\flat}$-, as in Safaitic, e.g., LP 736, and in Sabaean Ja 2561/2 and 2913c (from J. $^{c}$Amûd al-$^{c}$Ayraf, southwest of Mârib). Finally, the description of the sword welcomely points out that "the edges bear signs of sharpening" (p. 136b), a pleasing - yet wholly unvoluntary or unpremeditated - confirmation of the preceding interpretation.



Nationalmuseet 14646 a

6 - Relative chronology of the designs.

Since the sharpening normally is a part of the overall making of a sword, the inscription with the name of the sharpener himself must be ascribed to the time of



the casting of the weapon. The edition deals with the dating of the inscription in two different places, viz pp. 146B and 151B. The vocabulary of pp. 144B-146B (by Buchwald), however, greatly differs from that of p. 136B (by Lassen). Below are the various statements in two columns, viz those of p. 136B on the left, and those of pp. 144-146 on the right. When necessary, critical remarks are added. The reader is kindly requested to carefully note the various statements quoted below so as to direct his attention to the incompleteness and vagueness of many of them which so often make it so difficult, if not impossible, to solve several questions.

| | |
|---|---|
| Fig. A1: unmentioned | unmentioned |
| The longitudinal section of the upper design; = C1; cf. fig. B1 = D1 for its extremities. | |
| Fig. A2: [caption of fig. 6: "geometric decoration on one side of blade"] | |
| "a decorative pattern of straight and zigzag lines on the other side of the blade (Fig. 6)" (p. 136B, ll. 28-29). | no nounterpart |
| "The upper left part ... has a horizontal line with seven small, vertical strokes above and four slanting strokes below" (ll. 29-32). | "The 'four-legged animal with a raised tail' (Fig. 6)" (p. 145A, ll. 22-23; 145B, ll. 10-11, and 146B, l. 5). |
| The writer fails to note that the first five (from the right) lines have each another shorter bar appended to the left of the top. | This is the left part of the design only. It is difficult to understand why the writer having set his mind on an animal did not think of an iguana because of its spiky dorsal crest. |
| "In the middle of the blade are two vertical lines, the left one terminating in an 'E' shape, the right in a half oval" (ll. 32-35). | no counterpart |

The writer fails to note that the two endings are at the lower end, not at the top; and also that the two lower endings are not connected with the two vertical lines. The design could represent the lower part of a forearm with, for the hand, four fingers, on the left, separated by three bars and the thumb, a vertical concaved line, on the right.

| | |
|---|---|
| "The right part of the pattern consists of parallel zigzags, the first stroke of which begins between the two vertical lines in | no counterpart |

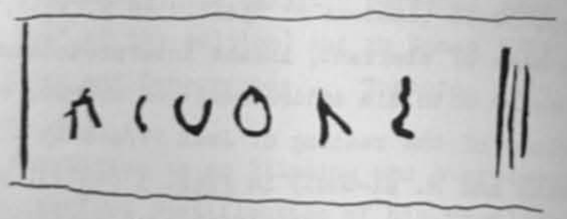
(c) Of course, the Arabic name Mas$^{c}$ûd is very well-known; cf., e.g., W. Caskel, Ǧamharat ..., pp. 401B-403A, with an impressive list of 80 references ... and of course - what else could he do? - Müller declares it to be the only possible vocalization of Sabaean ms$^{c}$d. Nonetheless, one finds "Messaad" and "Messad" in Vocabulaire ... des noms des indigènes ... de l'Algérie, Alger, 1885, p. 241A, or 1891, p. 285. What a shame that those Arabs could not have consulted with Pythonic Müller and did not know their native language well enough to avoid his disapproval!

(d) Müller's blindness also prevented him from being aware of his Janus-faced attitude. ms$^{c}$d is vocalized "Mas'ad" by A.R. al-Ansary, Qaryat al-Fau..., Riyadh, 1982, p. 143, no. 5. Müller's inborn reflex of flattering people whom he may eventually need to foster his future rang the alarm bell. So, for al-Ansary's vocalization of "Mas$^{c}$ad" and of "$^{\flat}$Arš," Müller put on his well-patted kid gloves and enchantingly states that these two forms "in Nordarabischen anscheinend nicht nachzuweisen sind" (p. 161). But, when he feels fully backed by the laudemus invicem club members, his vitriolic fanaticism fiercely bursts out, viz he refers to my vocalization of the name as "Musa$^{c}$ad" which is maintained here, as "allerdings mit der völlig unarabischen Vokalisierung Musa$^{c}$ad" (p. 161). For Müller and al-Ansary's "ms'd," cf. below, p. 126.

c - The dextrograde text of the Nationalmuseet 14646 a reads as follows:

→ |$^{\flat}$rhf sl||| Sayl has sharpened [it].

My tracing from my diapositive (without a scale) is reproduced on the right. As in many ancient texts, the complement of the verb is the object bearing the inscription. - $^{\flat}$rhf, cf. Arabic rahufa "it (a sword or a thing [of any kind]) was, or became thin, and slender" = rahafa, which has the same meaning as the 4th form $^{\flat}$arhafa "to make it (sword or a thing [of any kind]) thin; sharp, or pointed; sharp, or keen" (cf. E.W. Lane, l.c., p. 1170A-B). Note the 4th verbal form in $^{\flat}$-, as in Safaitic, e.g., LP 736, and in Sabaean Ja 2561/2 and 2913c (from J. $^{c}$Amûd al-$^{c}$Ayraf, southwest of Mârib). Finally, the description of the sword welcomely points out that "the edges bear signs of sharpening" (p. 136b), a pleasing - yet wholly unvoluntary or unpremeditated - confirmation of the preceding interpretation.



Nationalmuseet 14646 a

6 - Relative chronology of the designs.

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the casting of the weapon. The edition deals with the dating of the inscription in two different places, viz pp. 146B and 151B. The vocabulary of pp. 144B-146B (by Buchwald), however, greatly differs from that of p. 136B (by Lassen). Below are the various statements in two columns, viz those of p. 136B on the left, and those of pp. 144-146 on the right. When necessary, critical remarks are added. The reader is kindly requested to carefully note the various statements quoted below so as to direct his attention to the incompleteness and vagueness of many of them which so often make it so difficult, if not impossible, to solve several questions.

| | |
|---|---|
| Fig. A1: unmentioned | unmentioned |
| The longitudinal section of the upper design; = C1; cf. fig. B1 = D1 for its extremities. | |
| Fig. A2: [caption of fig. 6: "geometric decoration on one side of blade"] | |
| "a decorative pattern of straight and zigzag lines on the other side of the blade (Fig. 6)" (p. 136B, ll. 28-29). | no counterpart |
| "The upper left part ... has a horizontal line with seven small, vertical strokes above and four slanting strokes below" (ll. 29-32). | "The 'four-legged animal with a raised tail' (Fig. 6)" (p. 145A, ll. 22-23; 145B, ll. 10-11, and 146B, l. 5). |
| The writer fails to note that the first five (from the right) lines have each another shorter bar appended to the left of the top. | This is the left part of the design only. It is difficult to understand why the writer having set his mind on an animal did not think of an iguana because of its spiky dorsal crest. |
| "In the middle of the blade are two vertical lines, the left one terminating in an 'E' shape, the right in a half oval" (ll. 32-35). | no counterpart |
| The writer fails to note that the two endings are at the lower end, not at the top; and also that the two lower endings are not connected with the two vertical lines. The design could represent the lower part of a forearm with, for the hand, four fingers, on the left, separated by three bars and the thumb, a vertical concaved line, on the right. | |
| "The right part of the pattern consists of parallel zigzags, the first stroke of which begins between the two vertical lines in | no counterpart |

the middle, the remaining six strokes occupying the right side of the design " (ll. 35-39).

It is much simpler to state that there are four horizontal zigzag lines composed of seven elements each, and that the first of each line (on the left) is engraved between two vertical elements of the preceding design.

Fig. B1: "a geometric design, which looks like bundled reeds, is seen on the flange of the pommel" (ll. 40-41). | "a delicate pattern of alternating horizontal and vertical lines" (p. 144B, ll. 1-3 from bottom).

The word "flange" is abusively used instead of "rounded surface." What the two authors describe is the geometrical design engraved on the pommel. It is very surprising that no photograph illustrates that "delicate, geometric design" of which the authors do not give even an approximate sketch. The pommel itself is described as "wedge-shaped" (p. 136B, l. 11), whatever the expression may refer to. The pommel is built up as a paw with three well-distinct toes, and between the hilt and the paw, there is one horizontal fold (not mentioned by the editors) representing some kind of bangle or anklet. Judging from the color slide at my disposal, the design is engraved only on the central, longitudinal section of the pommel; nothing is seen on the two lateral sides. It is quite incomprehensible that that very design is represented in fig. B1 as a mixture of 6 hyphens and 5 (more or less traced) dots making a bottomless square, ; and it is even more astonishing that the design is completely eliminated from fig. D1. Judging from the same slide, the design seems to be composed of several pairs of closely parallel lines perpendicular to the sides of the pommel, and the spaces between those groups of lines are divided by short vertical lines in various numbers depending on what space they are. A special close-up is needed to make a reliable tracing of this complex design; B1 = D1.

Fig. B2: "an inscription (Fig. 4) on one of the hilt flanges consisting of six signs bounded by vertical lines on each side" (ll. 18-20). | "The inscription consists of six letters bounded on either side by vertical lines (Fig. 4)" (p. 145B, ll. 15-16).

"Flange" is abusively used instead of "side." - The verb "bound" in both texts suggests that one statement was inspired by the other, but it remains unknown which was the first.

Fig. B3: "an incised fish on the lower part of the hilt (Fig. 5)" (ll. 20-21). | "the fish (Fig. 5)" (p. 145A, ll. 6-7).

[cf. caption of fig. 5: "fish decoration"].



This section of the sword, which is the cross guard, is described as "a broader, protrusive part at the lower section of the hilt" (p. 136B, ll. 11-13). Such a description is inaccurate: the cross guard is neither "broader" nor more "protrusive" than the pommel. When the blade rests flat on the ground, as in fig. 2A, the whole design shows two biconcave lenses, one very narrow on the outside and a broad one on the vertical axis of the blade. When the blade lay on its narrow side, as in fig. 2B, the design is that of a tall, narrow, upside-down isosceles trapezium with rounded angles.

Fig. C1 = A1.

Fig. C2: "a rhombic design on one side of the blade just below the hilt, possibly representing a stylized fish with head facing the sword point and tail turned towards the hilt" (ll. 24-27). | "The 'rhomboidal fish pattern' on the sword blade opposite the four-legged animal" (p. 145B, ll. 9-11) = "the other fish" (p. 146B, l. 4).

Judging from all the differences in interpretation listed above and below, it is certain that the two above interpretations come from the same source. However, the interpretation involving a fish is farfetched: there is no detail which even remotely reminds one of a fish. It looks as a checkered design composed of lozenges on their vertical axes and arranged to form an upside-down, almost isosceles triangle suporting a rectangle. Here also, a close-up is necessary.

Fig. D1 = B1.

Fig. D2: "an incised 'Y' shape opposite the inscription on the other hilt-flange" (ll. 22-23). | "the 'whip' on the hilt is entirely different (Fig. 2)" (p. 145B, ll. 3-4), but only "whip" in p. 146B, l. 2.

The writer should have specified that his "'Y'" is upside down and was given a stilt to stand on. The design is understood as the calligram of the sharpener = 14646 b. Here again, "flange" is abusively used instead of "side." | It is difficult to understand what gave the writer the idea of a "whip," and especially he should have explained what could have been the reason for engraving a "whip" on the hilt of a sword? Besides, the strongly arched main section excludes the flail interpretation.

According to p. 146A (ll. 6-7), "the study of the decorations and the inscription leads to the conclusion" that all the designs added to the sword belong to three different periods as follows:

| dating | designs | reasons given |
|---|---|---|
| "the band on the pommel is contemporaneous with the manufacturing of the sword" (p. 146A, l.7 - B, l.2) | fig. A1 = C1 + B1 = D1 | "tool ... sharp" (p. 144B, last line); "the hand ... welltrained [sic!] (pp. 144B-145A). |
| — — — | — — — | — — — |
| "by an early owner of the sword, but probably not by the craftsman responsible for the manufacturing of the sword" AND "these decorations [= fig. B3 and A2, left only] and the inscription [= fig. B2] must be ancient" because of "their extensive and genuine corrosion attack..." (p. 146B, ll. 6-10). | fig. B3 | "coarser tools ... more brute force than" in fig. B1=D1 (p. 145A, ll. 9-10); and two chisels (p. 145A, ll. 11-12). |
| | AND | |
| | fig. B2 = Ja 3232a | "at least three different tools" (p. 145B, ll. 25-26) AND "most important is a crescent-edged tool" (p. 145B, l. 27) used in "the eye of the fish" (p. 146A, ll. 3-4) = fig. B3. |
| | AND | |
| | fig. A2, left only | "could have been done by the same hand that made the fish" (p. 145A, ll. 23-24) = fig. B3. |
| — — — | — — — | — — — |
| "rather recent date, perhaps this century" (p. 146B, ll. 3-4). | fig. D2 = Ja 3232b | "entirely different ... It is cut ... with a triangular cutting edge" (p. 145B, ll. 3-6). |
| | AND | |
| | fig. C2 | "apparently made by the same hand" (p. 145B, l. 11), as fig. D2. |

The overall argumentation of the writer leans much too heavily on the difference of tools and hands. Such a position is disputable because, on the one hand, "at least three different tools have been used for the letters" (p. 145B, ll. 25-26) of fig. B2 and, on the other, the writer has never even stated the impossibility, which is unprovable, of having several workers and/or engravers busying themselves with the making of a single bronze object. As a matter of fact, three workers are already certain, viz the caster, the sharpener and the engraver of the inscription since it cannot be posited that either of the first two above-listed workers was professionally capable of engraving not only the lettering of fig. B2, but also, as the edition pointed out, the fish of fig. B3. These two figures are so beautifully executed that a professional engraver had to make them. It should be pointed out here that the writer's reasons for ascribing fig. B3 to a date different from that of fig B1 = D1 is, in my opinion, invalid because "coarser tools", viz less refined or less professional, will affect and



alter the quality of workmanship and the use of human strength. Furthermore, the simple design of fig. D2 is perceived as the calligram of the sharpener, a1 = Bayl, a tall ( = 1, of which the upper half is ∧ = a. It does not seem superfluous to point out that the calligram is dextrograde, as the inscription itself. Was the remaining design, fig. C2, also engraved by the sharpener? He could have done it with all the tools he had: both fig. C2 and D2 have in common very ordinary lines and no elaborate pattern of any kind. No comparison is unfortunately possible with the design of the pommel, fig. B1 = D1, of which, as indicated above, no photograph, no facsimile are given in the paper. The difference of tools is secondary, as the use of any given tool depends upon all sorts of circumstances, and some of them are quite independent from the worker. For the letter 1, cf. above, p.12, Ja 3263.

The author invokes the "corrosion attack" process in fig. A2, B2 and B3 to prove their ancientness. This seemingly strong argumentation is rather vague and inadequate because their ancientness does not necessarily imply, as the argumentation would like the reader to believe, "an early owner of the sword," viz the three items should have been produced by a single individual. They would still be "ancient" had they been made by three different persons living during three different periods in "ancient" time. However, fig. B2, the main inscription, must have been made at the time of the making of the sword because, as shown above, the text was ordered by the sharpener. Therefore, even the edition would have no difficulty in ascribing fig. A2 and B3 to the same period. Incidentally, fig. A2 is reduced by the writer to its left horizontal third (cf. above, p. 68, right column).

The dating of fig. D2 and C2 on the sole basis of the edition's reason (cf. above, p. 68) is difficult to accept. The study should first have provided the reader with the most pertinent information, viz the accurate comparative appraisal of the degree of corrosion not only for fig. A2 and D2, but also for fig. B1 = D1. Also the question must be raised as to what reason, what purpose, what objective any late comer of "perhaps this century" (cf. above, p. 68A) might have had to add the insignificant fig. D2 (when isolated from fig. B2) and the crude fig. C2? Even would such a more or less contemporary worker have had the idea of somehow imitating or duplicating fig. A2 in fig. C2, and fig. B3 in the stick fig. D2, there can hardly be any doubt that that man did not exist unless he be demented enough to attempt a dream he manifestly was unable to materialize. And, a non-professional engraver would surely never have had any desire of engraving anything.

Summarily, the inscription, fig. B2, being the official testimony of the sharpener of the sword himself, was chiseled by a professional engraver because of

the neatness and the beauty of the lettering. According to the edition, the same engraver also is responsible for the zigzag complex of fig. A2, and the fish of fig. B3. The testimony of the text leaves no doubt on the contemporaneity of all those three items with the making of the sword itself; and the edition has no difficulty in ascribing fig. B1 = D1 to the same period. Furthermore, there is no detail in the edition against ascribing fig. D2 to a non-professional engraver, viz the sharpener, who may also be responsible of fig. C2.

7 - The dating of the inscriptions 14646 a and b.

On the question of the absolute dating of the complex, the edition states that "it is probable that the inscription was engraved during the first part of this period, i.e. c. 900 - 800 B. C." (p. 151A, ll. 12-14); "this period" refers to the dating of "roughly from 900 B. C. to 600 B. C." (p. 151A, ll. 10-11) for the "Proto-Arabic inscriptions." The first part of this frame comes from G.E. Mendenhall's theory, referred to by W.W. Müller (p. 150A, ll.16-18), which keeps on ignoring, among other things, the information of BASOR, No. 172, December, 1963, pp. 41-54, to the effect that that kind of lettering goes back much farther than the 9th century B.C. The second part of the frame comes from the writer's endorsement of J. Pirenne's mini-chronology, which I have opposed ever since the beginning and which has not (of course) resisted the onslaught of the C14 test dates, but which until very recently was still stubbornly maintained by C. Robin. Furthermore, the dating of "c. 900 - 800 B. C." does not take into account that the edition does not ascribe the text to the time of the making of the sword, but only to "an early owner of the sword" who could easily have belonged to about 600 or 500 B.C., or even later: that would still be "early." On palaeographical grounds, the inscription of the sword may be referred to the 10th century B.C. (cf. above, p. 42).

— — — —

(May 12, 1991; revised in June 1994)

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## III - The inscribed Qatabanian antiquities of the Yale Babylonian Collection (YBC).

### A - General remarks.

F. Renfroe's paper on a few small Qatabanian antiquities originating from the late Kaiky Muncherjee's collection and stored in the Yale Babylonian Collection apparently since 1926 (cf. Le Muséon, 103 [1990], pp. 155-165) is characterized by an empty simplism based on a mere embryonic knowledge allowing the author's imagination to wander freely at the expense of the texts. The only value of the paper lies in its photographic documentation, but even that value is greatly reduced by the absence of scale in any of the 12 photos, and the color of the material of each antiquity is not given either.

The series is composed of eleven objects YBC 2421 - 2432 (there is no 2424?). Renfroe does not seem to be able to follow the numerical order as he begins with no. 2425 and ends up with no. 2423. No reason is given for this illogical treatment or the skipping of no. 2424, and none presents itself during the reading of the paper, with the exception that nos. 2431 (pp. 162-163) and 2423 (pp. 163-165) were left aside as the last two objects to be treated apparently because the author needed the exercise of the nine other antiquities to convince himself that his knowledge was up to those antiquities, each of which contain more than 3 words (viz a personal name + the relative pronoun + a clan/family name, as in nos. 2425 and 2432) or 2 words (viz a personal name + a family/clan name, as in nos 2421, 2426, 2427, 2428, 2429 and 2430) or even 1 word (viz a personal name as in no. 2422). That the author had become convinced of his professional ability is stated out in his assertion proudly presenting those 11 inscribed antiquities "to the scholarly public for the first time" (p. 155). Scholarship has nothing whatsoever to do with the gibberish dilettantism immortalised by the soap opera-like treatment of the last two antiquities, viz nos. 2431 and 2423 (pp. 162-165). Finally, none of the author's measurements can be checked because of the lack of scale on the photographs.

The first seven antiquities belong to the well-known type of proscynema bases. In each case, Renfroe uses the expression "socketed ... base" (e.g., pp. 156, 157, etc) without paying any attention to the mortise (form and measurements) of the top side. Renfroe also mentions that the antiquity is "polished on four surfaces" (cf. nos. 2425, 2427 and 2428) or only "two" (cf. nos. 2426, 2429 and 2432). Beside the front side which is always polished, Renfroe nevers specifies which of the five other sides is/are polished. The next two antiquities, nos. 2421 and 2422, are

the same type of bases with the remains of feet in each case. Here again, Renfroe mentioned "polished on five surfaces" (p. 161, twice) without specifying which of the five sides (beside the front) is polished.

No inscription on those seven antiquities presents any difficulty but Renfroe's commentary on nhym of no. 2432/1 (pp. 160-161) already presents the reader with a clear example of the author's simplism. The name nhym of Ja 299/1 is correctly understood in the edition as a qutayl derivation on the basis of the Sabaean names nhms (person) and nhmt (clan). Not only Renfroe distorts the clan name nhmt to "nhyt" (p. 161), but he also rejects the above etymology on the basis of the feminine nhym of no. 2432. His absurd reasoning is unworthy of consideration. Instead, he should have mentioned that his etymology had already been suggested by G.L. Harding, An Index..., Toronto, 1971, who also connects nhym to the root nhw (p. 606); and especially the Sabaean masculine nhyt of CIH 434/15-16.

All the names mentioned in the inscriptions dealt with so far are well-known, and even then Renfroe does not always give any reference, as in ʿmkrb of no. 2428/1, nʿmm of no. 2429, and ġytm of no. 2421. The only new name demanding some study is bhym of no. 2429 which Renfroe vocalizes "Bahîyum" with the comment that "no explanation of the name suggests itself" (p. 159). The author does not justify the reduplication of y which is demanded by his vocalization. His comment, which remains unexplained, is meaningless except for stating that its author does not know what to do with the name. It would be very easy to refer to name to Arabic bahâ (o). But, this root is a secondary form of Arabic bâha (o) (cf. R. Blachère et al., Dictionnaire arabe-français-anglais (Langue classique et moderne), Paris, I [1967], p. 416B, and II [1970], p. 999A, respectively; and G.W. Freytag, Lexicon arabico-latinum, I, pp. 92A and 171A). E.W. Lane, An Arabic-English Lexicon, has an entry for bwh (p. 274A) but none for "bhy" (p. 160C), and the Thamudic verb bh = bwh is found in Hu 512 (cf. my Miscellanées d'ancient arabe, VII, p. 134, note 81) and WiTh 64 (cf. l.c., XVI, p. 247). Furthermore, the root "bhy" is not attested in North-Arabian onomastics. A. van den Branden, Inscriptions thamoudéennes, Louvain, 1950, p. 522B, refers "bhy" to "HU. 512." But the reference is erroneous. The name bhy is also read in Ph 275 w (cf. my Thamudic Studies, Washington, 1967, pp. 76 and 86A). But this copy of Philby can hardly be considered as more trustworthy than any other. There seems, therefore, to be no choice left except to understand the name bhym as a qutayl nominal derivation from the root bhm ; cf. Arabic bahm "any plant without a stem, stemless" (cf. R. Blachère et al., l.c., I [1967], p. 415, and G.W. Freytag, l.c., I, p. 91B); thus bhym Buhaym. The Safaitic "bhm" or "khm" is read in WH 2487 (cf. F.V. Winnett and G.L. Harding, Inscriptions from Fifty Safaitic Cairns, Toronto, 1978, pp. 557B and 605A, respectively). But, the name reads kym (cf. my Miscellanées..., XIII, pp. 25 and 223). The root bhm is nonetheless attested in the Safaitic place name bhmt of WH 1771 (cf. my Miscellanées..., l.c., pp. 211-212 and 244).



There remain only two more important antiquities, viz nos. 2431 (pp. 162-163) and 2423 (pp. 163-165). Renfroe devotes to their translation and commentary 6 (six) times more lines than for the 9 other antiquities combined. Such a verbal profligacy does not deserve to be read and even less to be discussed. To be sure, Renfroe did his utmost to put on the best possible show of his knowledge ... and his success is complete: it would indeed have been difficult to give a better proof of his basic ignorance of things Qatabanian, and his mistreatment could hardly be equalled.

(1) He does not even seem to be able to recognize what these antiquities really are, viz nos. 2431 and 2423 are described, respectively, as a simple "block" (p. 162) and a "cup" (p. 163), although they manifestly are a votive plaque and a saucer. For the latter, cf. already A. Legrain, in AJA, 38 (1934), p. 337. Since Renfroe does not seem to know how a "cup" really looks like, he might profitably consult J.-C. Corbeil, The Facts on File. Visual Dictionary, New York, 1986, p. 213 (upper half) and 585 (lower left); or, if he prefers, Oxford-Duden Bildwörterbuch. Englisch, Mannheim, 1981, pp. 62 (no. 27), 89 (no. 29) and 569 (no. 69).

(2) He completely misses the simple phrasaeology of no. 2431 and invents the new meaning of "statue" for nfs person; cf. above, p.63, for W.W. Müller's phantasy.

(3) He does not know that the stippling, far from being "rather unusual" (p. 164), is a well-known engraving process especially used in Qatabân and more extensively in ʾAwsân. Neither does he know that the sections in stippling and those in linear tracing are not normally to be united in a single text.

(4) Renfroe succeeds in misreading 3 letters in Ja 3226.

B - Two YBC inscribed antiquities.

Ja 3225 - The left-hand corner of a votive albaster plaque. - YBC 2431.

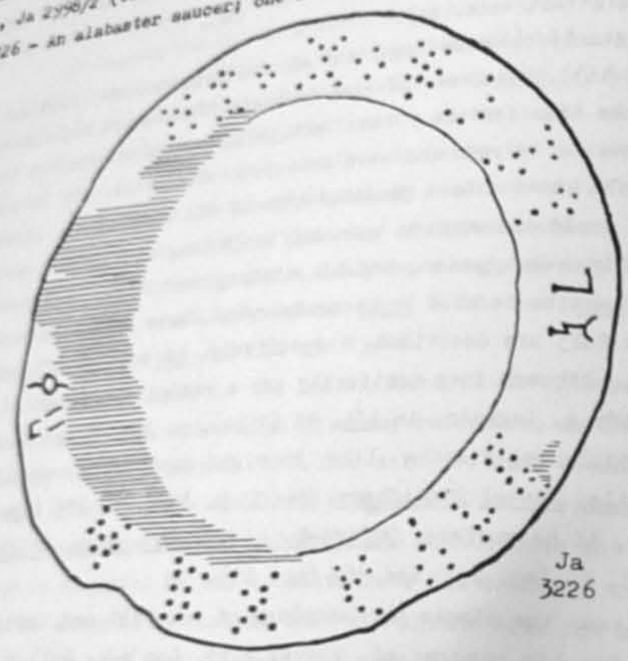
...]krb/sqny⌜/⌝yġr/rtd/nfs⌟[s/w...

...]karib has dedicated [his wife] Yaġir. He has entrusted himse[lf and ... to the care of ...]

yġr: the second letter is certain: the lower extremity of the diacritical appendix is still on the stone. Cf. the Thamudic personal name in JsaT 538, and the Sabaean plural verb yġrw in Ja 643/32, contrary to W.W. Müller in NESE, III, p. 136. - rtd without being introduced by the conjunction w and is especially attested in Qatabanian. - yġr may be considered as the author's wife; cf. already Ja 3064/1, 3069 and 3073 (to be

published); cf. below, pp. 103-104, the commentary on mlḥmw of Turner 1/1. - nfs, cf., e.g., Ja 2998/2 (to be published).

Ja 3226 - An alabaster saucer; one large spot damaged on the rim. - YBC 2423.




The engraving of the two inscriptions seems to have been done, strange though as it may be, after the left side of the saucer had been damaged; all letters seem complete, and qb and hl, on the one hand and, on the other, h and l have the same height.

A - A long line in stippling and in two sections of two words each engraved diagonally from each other:

| | | |
|---|---|---|
| yšrḥ't ʿṣb | 1 | Yašraḥʿat, chief of |
| ʾmt khlm | 2 | the maidservant of Kahilum. |

yšrḥ't, cf. the more frequent form yšrḥ'tt of Ja 2974/1. - ʿṣb, cf. Arabic muʿaṣṣib "chief, (one) made chief" (cf. E.W., Lane, l.c., p. 2060B). - khlm, e.g., RÉS 3902, no. 112.

B - At both ends of the axis perpendicular to that of the preceding text.

hl qb
Hâl [of the family] Qabb.

or

qb hl
Qabb [of the family] Hâl.

qb, cf. the Thamudic personal name, e.g., in Hu 411/2, and qbb, a proper name in Sabaean CIH 30/3-4, and name of a person in Safaitic CIS 466; for Thamudic WiTa 43, where the edition reads "qbb," cf. my study in Orientalia, 40 (1971), p. 488. There



is no way of knowing which one of the two names comes first.

C - Complementary documentation.

The stippling is an engraving process used most frequently to trace short identification cards on small utensils, e.g.

(1) on a saucer. In this case, the words are engraved on the rim, and the elements of the text are disposed on opposite sides of the diagonal; it is, therefore, impossible even at first sight to determine which name is of a person, and which is of a family, as in the present Ja 3226. Here follow a few examples:

KM my 340 = AM 60.1261 (?) (cf. my Miscellanées..., II, p. 132) with ʿbkhl and ḫwtʾl; KM my 308 (cf. below, KM my 341) = AM 60.998 or 60.10003 (cf. my Miscellanées..., l.c., pp. 128-129) with ʿmytʿ and ʾbkhl;

AM 61 (a grayish alabaster saucer with white and reddish streaks; broken in two parts) with the same pair of names, ʿmytʿ and ʾbkhl, as above, in KM my 308;

KM my 318 with the names ʿtlm and ʿmdʾbn; the last three letters form a calligram;

KM my 321 with the names ʿmwtn and ḫwmʾl, as in KM my 304 A (cf. below, p. 77);

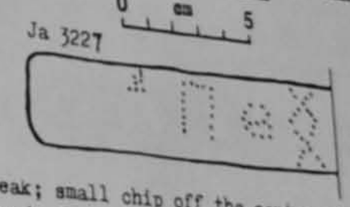
KM my 352 with the names klb and ʾḫwym;

(2) on a small vase: Ja 2221 (cf. my Miscellanées..., l.c., p. 55 and pl. 3);

(3) on a perfume bottle; e.g. KM my 350 with the names klb and ʾḫwym, as in KM my 352 (cf. above). These two antiquities are AM 60.1044 and 60.1107 (cf. my Miscellanées..., l.c., p. 130);

(4) on a small bowl; e.g., RÉS 4254 and A. Jamme, in Le Muséon, 65 (1952), p. 155B;

(5) on a flat box; Ja 3227 = KM my 307, with the name śwbʿ[1] Śawbʾi[1]; the name was left unfinished:




both the lower half of dextrograde ʿ and the whole letter l were not engraved. Same type of antiquity as in AM 60.995 = RÉS 3902, no. 28[1], viz a flat box with two circular cavities on top; lightly yellowish alabaster with white streak; small chip off the center of the left lateral side. Top: rectilinear long sides and convex lateral sides; two circular cavities of 5.5 cm in diameter, 1.9 cm deep, and separated from each other by 0.8 cm. Front: length (at the bottom): 12.6 cm; height: 2.7 (right) and 2.8 (left) cm; text: 7.8 x 2.5 (in ś) and 1.7 (in b) cm.- My facsimile is on the right.

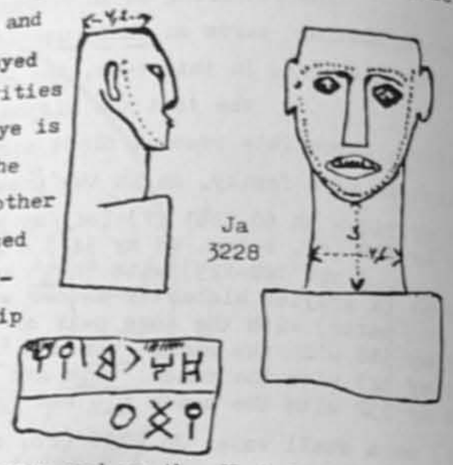
śwbʿʾ[1], cf. in Qatabanian śwb of Ja 2051/1 (from Mukéràs) = twb, e.g., of Ja 1435/1 (from Mukéràs) and Sabaean Ja 2150 b/1; cf. also the Qatabanian twb[.?] of TC 1511 and twbʾl in Ja 867/1.

The stippling is also used in the statuary to engrave different parts of the image; e.g.

(1) on a head to represent the beard; e.g., Pennsylvania 30-47-20 (cf. A. Legrain, in Gazette des Beaux-Arts, 56 [1934], p. 67, no. 9);

TC 1776 (cf. R.L. Cleveland, An Ancient South Arabian Necropolis..., Baltimore, South-Aden (cf. Le Muséon, 45 [1952], p. 296 and pl. 7, bottom); and the ʾAwsânite statues RÉS 3885, 3886, 3887, 3888 and 4070;

(2) on a head to represent beard and eyebrows; e.g., Ja 3228 = AM 35, a monolithic alabaster proscynema composed of a head and an inscribed base. Some of my notes were destroyed by the 1976 fire. The lozenge-shaped eye cavities are at different levels, viz that on the left eye is about half-way below the other; the cavity of the left eye shows 4 small, circular mortises, the other only two; the eyebrow on the right eye is composed of 9 tiny holes, but the other only has 6. Thickness of the block: 4.2 (top of the head), 5.6 (tip of the nose), 5.3 (chin), 4.5 (neck), 5.6 (top of the base) and 5.65 (bottom of the base) cm. Front of the base: the whole upper edge is destroyed. Width of the head on the ears, above the ears and on the flat top: 6.5, 6 and 5.7 cm, respectively. Thickness of the block on the flat top, the tip of the nose, the chin, the neck and the base: 4.2, 5.6, 4.5, 4.5 and 5.6 cm, respectively. Text: 1.1: 6.3 x 1.75 (m) and 1.3 (r) cm; 1.2: 3.3 x 1.5 (y) cm. - The three facsimiles of my fieldbook are reproduced on the right. - The inscription may be read as follows:




| | | |
|---|---|---|
| dhrm/yʿhʾ | 1 | Dahrum [of the family] Yuh- |
| ysʿ | 2 | aysiʿ. |

dhrm, a personal name, e.g., in Sabaean CIH 529/1, and name of a Qatabanian family in Ja 1125, and of a Sabaean clan in my Sabaean Inscriptions from Maḥram Bilqîs (Mârib), Baltimore, 1962, p. 413A; the h is dextrograde. - yhysʿ, cf. yhytʿ, the name of a Qatabanian family in Ja 2386/1, and of a person in TC 1591; cf. also hwfʿśt of Ja 2490 k and commentary in my Miscellanées..., III (1972), p. 97.

(3) on a head to represent moustache, eyebrows and beard; e.g.,

TC 1361 (cf. R.L. Cleveland, l.c., pp. 6B-7A and pl. 8, top, and 9); and RÉS 3902, no. 52 = Ja 71 (cf. my paper in Le Muséon, 65 [1952], pp. 104-105);

(4) on a head to represent beard and moustache; e.g., RÉS 4237 (cf. my paper in BiOr, p. 153B);

(5) on a head to represent beard, moustache and imperial; e.g., RÉS 4240 (cf. my paper in BiOr, l.c.);

(6) on a head to represent beard, moustache, imperial and goatee; e.g., RÉS 3902, no. 8 = Ja 60 (cf. my paper in Le Museon, l.c., p. 98);



(7) on a head to represent the hair; e.g., AM 37, a whitish alabaster proscynema representing a standing man with a walking stick in the left hand; the figure is in the round against the third hollowed central cavity; 10.3 (bottom) and 12.5 (top) x 27 (right side) cm; thickness: 4.2 (top) and 4 (bottom) cm; the hair is marked by 29 small holes in 3 series. The 3 levels in the center are, respectively, 0.3, 0.4 and 0.5 cm below the outside frame; and the figure's thickness is 0.6 cm, thus 0.1 cm more than the bottom of the inner 3rd hollowed space. - My facsimile is on the right.

(8) on the chest to represent the upper edge of a female dress; e.g., TC 2530 (cf. R.L. Cleveland, l.c., p. 26B and pl. 51).

The dots are also commonly used on the coins; e.g., they represent the hair of the human head (e.g., VL no. 15), the hair on the cranium and the temple (e.g., BM XI-XII, no. 40), an ornamentation circling the human head (e.g., Ja 3156 and 3157) and the owl's body (Ja 3159); all the preceding references are to be published.

The stippling is also used in the engraving of small texts and petroglyths, such as those found in Ḥaḍramawt (cf. G. Caton Thompson, The Tombs and Moon Temple of Hureidha (Hadhramaut), Oxford, 1944, pl. 69 and 70; cf. my remarks in my Miscellanées..., I, p. 74), probably due to the ʾAwsânite influence.

The two engraving processes, stippling and linear, are also used simultaneously on small utensils; e.g.,

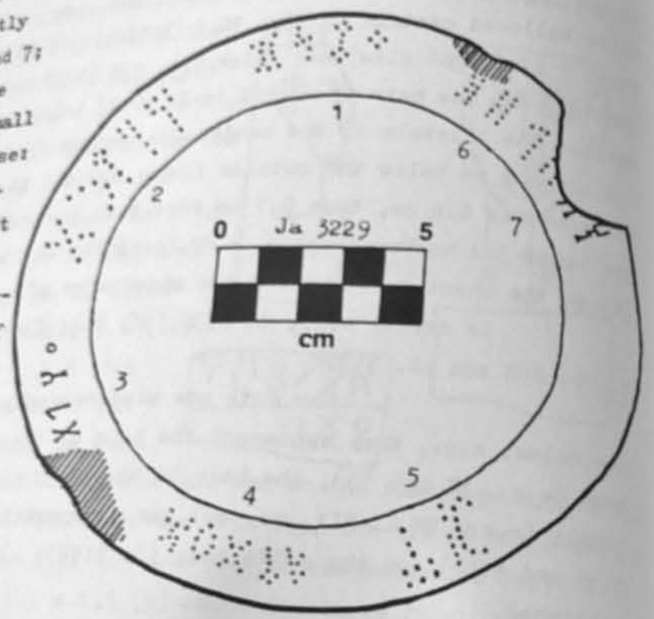
(1) on a saucer, as RÉS 4253 where the first name is ʾbkrb in stippling (contrary to my paper in BiOr, l.c., p. 154A, which gives "ʿmkhl" from another saucer), the second, d(s)mg in linear (cf. l.c.).

KM my 304A with ʿmwtn and ʾwsʾl in stippling, and B: ʾl and ⌜śʾlʿ⌋ in linear; the stippling is as in KM my 321 (cf. above); = AM 60.1030 and 1034; cf. my Miscellanees..., II, p. 129;

Ja 3229 = KM my 333, with even more writing than YBC 2423 = Ja 3226; cf. below.

(2) on a small bowl; e.g., KM my 341 with A: ʿmytʿ, ʾbkhl and 2 cm to the right of ʿmytʿ an incomplete word in stippling: ⌜ʾ⌝m⌜t⌝, and B: ʾlf in linear. The upper half of the first letter of the noun shows only a single dot at the top of the letter and in the axis of the lower vertical staff; thus, either ʾ or k. The letter t is represented by a single aslant bar going from the upper right to the lower left. Text A may be read as follows: ʾbkhl ⌜ʾ⌝m⌜t⌝ ʿmytʿ ʾAbkahil, maidservant of ʿAmmyataʿ. The two names of text A are also found on two saucers, KM my 308 and AM 61 (cf. above, p. 75). The three objects, AM 61, KM my 308 and 341, may be considered as belonging to the same woman. - B: ʾlf ʾÂlif; cf. Safaitic CIS 12.

Ja 3229: a saucer atop a base, of yellowish alabaster whith white and brownish streaks; intact except for a large chip (3.5 x 1.5 cm) broken off after the engraving and which has partly destroyed one letter in nos. 6 and 7; a smaller one about 1.5 cm to the left of the first, and another small one diagonally from the first. Base: diameter at the bottom: 7.5 (exterior) and 4.6 (interior) cm, and at the top: 5.7 cm. Total height: 6.5 cm. Upper part: diameter: 15.5 (exterior) and 11.5 (interior) cm; depth of the cavity: 2.4 cm. - KM my 333.




Measurements of the seven names:

1: in stippling; 3.2 x 1.2 cm; at 0.3 cm from the exterior edge;
2: in stippling; 4.5 x 1.5 cm; at 2.2 cm from no. 1;
3: in linear; 2.7 x 0.8 cm; at 2.5 cm to the left of no. 2, at 0.7 cm from the exterior edge in h, and 0.5 cm in t;
4: in stippling; 3.4 x 1 cm; at 3.5 cm to the left of no. 3; at 0.7 cm from the exterior edge;
5: in stippling; 2.3 x 1.5 cm; at 1.9 cm from no. 4; at 0.5 cm from the outside edge;
6: in stippling; 3.2 x 0.9 cm to the right of no. 1; at 0.9 cm from the outside edge;
7: in linear; 1.2 x 1 cm; at 0.4 cm to the right of no. 6; at 0.8 cm (at h) from the outside edge.

The seven names are distributed in three groups depending on both the engraving process and the letter height. - My tracing is on the right.

A = nos. 6 + 1 + 4: ⌜k⌝lbm ʿtnm ʾftym  Kalbum [of the family] ʿAtnum ʾAftayum,
or 4 + 6 + 1: ʾftym ⌜k⌝lbm ʿtnm  ʾAftayum [of the family] Kalbum ʿAtnum.

klbm, e.g., Ja 244, Ḥaḍrami CaTh 67 b/1, and Sabaean Ja 563/2. - ʿtnm, cf. ʿtn in the Sabaean graffiti from southern Saudi Arabia in RSO, 32 (1957), p. 560, and also in Thamudic Hu 303. - ʾftym, as in RÉS 4278/1.

Note on RÉS 4278. - RÉS's judgment of RÉS 4278, viz "Faux; peut-être une copie moderne d'un original authentique" (cf. VII [1950], p. 177), is gratuitous and remains unjustified. Because of the preceding judgment, neither RÉS, VIII (1968), p. 131, nor A. Avanzini, l.c., II, pp.16-17, do list ʾftym! But, by pure illogism, RÉS, VIII, p. 198A, lists the second name of RÉS, viz mʿd, without any remark in p. 269, and no mention of ʾftym in p. 131B. RÉS copies almost verbatim M. Cohen's commentary (cf. Documents sudarabiques, Paris, 1934, p. 13). But, that it is a "statuette ... grossière" does not make it a fake or a modern copy of an original. Many small objects found in South-Arabia undoubtedly are coarse and rough; cf. the stone animal of photograph 164 of C. Rathjens - H. von Wissmann, Vorislamische Altertümer, Hamburg, 1932, p. 205, bottom right, and especially the crude trunk of a clay woman unearthed at Qaryat al-Fāw (cf. below, Ja 3240). RÉS further notes that "Cohen n'a pas retrouvé ce monument au Louvre. La copie faite par Mayer Lambert porte la cote: Louvre 53." How can the reader understand and justify that G. Ryckmans, the redactor of RÉS, failed to inform his reader of the perfectly clear statements contained in Cohen's publication, which reads as follows:

"Le socle porte l'inscription: 𐩱𐩲𐩧𐩣𐩵
𐩣𐩧𐩱

La statuette a été quelque temps en dépôt au Louvre en 1926 ou 1927; j'ai pu y vérifier l'exactitude de la copie de Mayer Lambert; l'objet n'a pas été acquis par le musée - et la photographie ne s'est pas retrouvée."



The facsimile does not show any unusual letter form; there is, thus, absolutely no reason to discard this antiquity from the Qatabanian (not "Sabaean" as in RÉS) corpus. For mʿd, cf. the family name in Qatabanian RÉS 3902, no. 91, and in Sabaean Ja 1008 b/2.

The inscription of RÉS 4278 reads as follows:

ʾftym  1  ʾAftayum
mʿd  2  [of the family] Maʿad.

B (of Ja 3229):

nos. 2 + 5: ḥgyrt ḥm  Ḥugayrat [of the family] Ḥamm;
or 5 + 2: ḥm ḥgyrt  Ḥamm [of the family] Ḥugayrat.

ḥgyrt, a qtylt derivation from the well-known root ḥgr; cf. the name, e.g., in Ḥaḍrami Hamilton 1/1, Minaean Ja 2269d, and Sabaean Ja 520/2. - ḥm, e.g., RÉS 4975 B, and a personal name, e.g., in Sabaean Ja 2553, Minaean JsaL 9, and Qatabanian Bellerby-Habban 4f.

C: nos. 7 + 3: h⌜b⌝ ʿhlt  Hayb [of the family] ʿAhlat;
or 3 + 7: ʿhlt h⌜b⌝  ʿAhlat [of the family] Hayb.

hb, cf. the Sabaean place name hbtn in Ja 745/11, and the substantive hbt failure in Ja 567/27. - ʿhlt, cf. the personal name ʿhl in Ḥaḍrami Ja 2949 a (cf. the commentary in my Miscellanées..., III [1972], p. 100); cf. also Safaitic ʿhl as a personal name in CIS 728, and a substantive in CIS 2177.

— — — — — —

Word list

ʾbhl (pers./fam.n.) AM 61
(fem.n.) KM my 341 A
ʾwsʾl (fem./fam.n.) KM my 321
ʾlf (pers.n.) KM my 341 B
ʾmt (fem.pers.s.) Ja 3226 A/2; KM my 341 A
ʾgwm (pers./fam.) KM my 352
ʾftym (pers.n.) RÉS 4276/1
(pers./fam.n.) Ja 3229 A
bhym (YBC 2429) p. 72
dhrm (pers.n.) Ja 3226/1
bgyrt (pers./fam.n.) Ja 3229 B
gn (pers./fam.n.) Ja 3229 B
hb (pers./fam.n.) Ja 3229 C
hl (pers./fam.n.) Ja 3226 B
yhyʿ (fam.n.) Ja 3228/1-2
yqr (fem.pers.n.) Ja 3225
yarbʿt (pers.n.) Ja 3226 A/1
khlm (pers.n.) Ja 3226 A/2
klb (pers./fam.n.) KM my 352
klbm (pers./fam.n.) Ja 3229 A
lkrb (pers.n.) Ja 3225
mʿd (fam.n.) RÉS 4278/2
nhym (YBC 2432/1) p. 72
nfs (pers.s.) in "[s: Ja 3225
-s (masc.pers.pron.) cf. nfs
sqny (v.; qny) Ja 3225
ʿhlt (pers./fam.n.) Ja 3229 C
ʿmdʾbn (pers./fam.n.) KM my 318
ʿmwtn (pers./fam.n.) KM my 321
ʿmytʿ (pers./fam.n.) AM 61
(pers.n.) KM my 341 A
ʿsb (pers.s.) Ja 3226 A/1
ʿtlm (pers./fam.n.) KM my 318
ʿtnm (fam.n.) Ja 3229 A
qb (pers./fam.n.) Ja 3226 B
qny (v.) cf. 4th form sqny
rtd (v.) Ja 3225
swb⌊ʾ⌋[l] (pers.n.) Ja 3227

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IV - The 1988 revised edition
of A.H. al-Scheiba's 1982 thesis
(rewritten in June 1995).

The case of the partial photo of Ja 576/1-16 (center) vindicating my original reading of ʿwfm (1.2) and ʿdb (1.11) against the members of the "béni-oui-oui" club (cf. my Miscellanées ..., X [1979], pl.5) stamped out all criticism against my reading. The case of the name of the city dll (1.4) is quite different. al-Scheiba's thesis written under W.W. Müller's despotic direction was published in 1982 as a separate booklet (siglum: ScOA); after some kind of new editing and updating, the same thesis appeared again six years later in Archäologische Berichte aus dem Yemen, Mainz, IV (1987) 1988, pp.1-62 (siglum: ScOAI). For dll, cf. below, p. 82.

The date of Archäologische...is given by W.W. Müller as "1987" in AfO, 35 (1988) 1990, p.432A, l.13. This date, which is also printed in Archäologische..., IV, p.iii (unnumbered) is wrong: "1988" is the right one as it is the year given by the same Archäologische ..., p. iv (unnumbered), bottom:

" © 1988 by Philipp von Zabern, Mainz am Rhein."

H. Kopp gives "Band IV (1987), herausgegeben von Jürgen Schmidt Oktober 1988" (cf. Jemen-Report, 20, 2nd half, (1989) 1990, p.42B, ll.6-7, from bottom). The month of this date, "Oktober," is also wrong as the copy of that volume was received "March, 18,1988." The year "1987" is that of the printing itself, but the publication took place in 1988, date of the copyright, as clearly printed by Archäologische ... itself (cf. above).

The facts presented below constitute a triptych made by W.W. Müller's voluntary omission and contradictory attitude, already stated in my 1994 text, and for which Müller, and he alone, is ultimately responsible.

-1- Contrary to the basic rule of objectivity in the printing business, ScOAI fails to inform the reader that the work had already been published as a 1982 Ph.D. thesis booklet. This inexcusable blunder is the cause of the deplorable triptych.

-2- One year later, H. Kopp provides the central crucial panel by characterizing ScOAI as "der bei Walter W. Müller angefertigten Dissertation" in Jemen-Report, l.c., p.42A, ll.21-22). The verb "anfertigen" "to make, to manufacture" opened the gate to, and justified, all kinds of interpretations and all the more so because ScOAI's introductory note (p.1A) VOLUNTARILY eliminated the word "Dissertation" (cf. ScOA, p.3, l.3 from bottom, = the thesis) and replaced it by "Arbeit" (cf. ScOAI, p.1, note *, l.2), a manifest proof, if there is one, of the firm intention of deceiving the reader. That was also the 1st change in the text of that note, the 2nd being found at the very end, viz

(Follows p.81A)

ScOAI: "sprechе ich meinen tief empfundenen Dank aus" is replaced by
ScOAI: "sprechе ich meinen tiefempfundenen Dank aus,"

an earthshaking change intended, no doubt, to drastically affect the foundation itself of South-Arabian research.

The balance of this paragraph is reproduced word for word in both editions, including the two comas. After that, ScOAI's introductory note repeats the initial phrase of the 2nd paragraph of ScOA's "Vorwort," dropping not only the 2nd statement of the paragraph on "Frau E. Richter" but also the very 1st paragraph of the same "Vorwort." All those changes manifest the fixed determination to adapt the new text to the new situation. Then, the substitution of "Dissertation" by "Arbeit" takes its full meaning: it is not an accident or a "lapsus calami;" it is one link in a carefully planned reviewing of ScOA's "Vorwort." Since, however, no updating can justify the preceding substitution, the question remains: what was the reason for such a replacement? The logical presumption would suggest that the reason was somehow connected with ABADY. Müller is defied to tell the reader what happened and to explain the reason. His explanation, however, belonging necessarily to "la politique du paillasson" or being a postmortem self-vindication, would automatically be impaired by an overwhelming suspicion.

-3- Another year later, in AfO, 35 (1988; published in 1990), p.432A, ll.13-15 from bottom), Müller authors the 3rd panel by proclaiming that ScOAI is "Im wesentlichen unveränderter Druck der Dissertation von 1982" (cf. also below, p.81C). As I already stated a year ago, how is it possible to publish such voluntary a falsehood? Such a statement necessarily presupposes changes that ScOAI's introductory note does NOT acknowledge. And how could it be that all those changes would exclusively concern non-essential questions? And if all of them are non-essential, some would still contradict the "unveränderter Druck" "Diktat." How could anyone call non-essential wrong dates, wrong conclusions, wrong references, etc? Finally, Müller's statement, which is a part of his journalistic report and could be the settlement of a personal affair that had to be made public to redress Kopp's summary and to set it in the correct perspective, actually is a postmortem remark (cf. above) which SHOULD HAVE ALREADY BEEN MADE TWO YEARS EARLIER in ScOAI's introductory note, the only right place, where it belongs.

Due to the confusion created by the signals presented above, the 1st edition of this section (July 1994) incriminated Müller of having "made, manufactured" (cf. above, Kopp's "anfertigen") the whole revision and repeated that position over and over again.

At long last, Müller's reaction came in a dispatch received last April 26, 1995, and containing two items.

a - Inside the envelope there was a letter which, upon being identified as such, was unwelcome and discarded at once as having been written by one of that "laude-

(Follows p.81B)



mus invicem" club, who during catastrophic circumstances, remained "cois," deluding themselves in their isolation booth with their "philanthropie désintéressée" (cf. Orientalia, 48 [1979], p.261, fn 1). I have once in the past dissected Müller's well-rehashed ritornello of lamentations and biased and erroneous statements strewn in AfO's columns, in my Miscellanées ..., XVI (1988), pp. 308-314. These notes were intended to complement the analysis of Müller's work in l.c., XIV (1985), pp. 132-175.

b - The 2nd half of Müller's dispatch was enlarged photostats of ScOA's revised pp. 65 and 72; they were indeed very welcome because of the indisputable "cuique suum." However, had it been only for Müller, these 2 photostats would have been discarded out of hand, as the letter had been, because they do not bear the proof of anything - the back of each photostat is a perfect blank - viz no signature; no date; no identification of any kind; two pages only instead of one for each of the pages mentioned in my study; and above all, no legal affidavit proving their authenticity. In spite of the conspicuous absence of the preceding warranties, I nonetheless accepted those photostats for the sake of al-Scheiba, for whom I am terribly sorry that he has been manipulated and taken advantage of by Müller.

Alas!, al-Scheiba is not alone in his situation, far from it. Another very recent example of Müller's misconstrued supervision, S.A. Tairan's 1992 thesis is presented in my review of JAOS, 114 (1994), p. 297B, as follows:

> "This disorganized, incomplete and partisan book on the old Sabaean personal names is filled with many biased and incorrect readings of texts, and statements that tend to distort the facts."

A few years earlier in 1986, there was another thesis by R. Hazim on the Safaitic theophoric names (( and more recently a short paper in Le Muséon, 104 [1991], pp. 355-358)). For Müller's incompetence in Safaitic, cf. my Miscellanées ..., XIV (1985), pp. 144-175. Hazim's work is even more incomplete, more nonsensical, more biased than Tairan's. As far as to review it, "le jeu n'en vaut pas la chandelle."

Therefore, those 2 photostats sent by Müller are considered as containing relevant facts, the respect of which has always been the basic principle of my research, as proven by every single page of my publications.

Two notorious examples among many others are the vindication of Mrs. Theodore Bent in the case of the Bethel stamp (cf. BASOR, 280 [1990], pp. 89-91 and my Miscellanees ..., XVI [1988], pp. 135-159 and 326), and especially the case of my Miscellanées ..., V (1974), the whole volume of which was destroyed because J. Euting's original copies had reached me and I could not go ahead with a study based on second-hand copies ... and I started all over again. Furthermore, it is my constant search for the facts that led me to identify the so-called Thamudic Ph 290 as the Aramaic funerary stela RiyâdhM 36, and also Glaser squeeze A 727 as related to the Bethel inscribed stamp unearthed by J.L. Kelso at Beitin (cf. already above). It is the same search for the facts that prompted me to add this short note on another South-Arabian antiquity. In the book Semitic Studies...edit. by A.S. Kaye, Wiesbaden, 1991, pp. 1370-1384, J. Ryckmans published with a particularly messy facsimile an inscribed copper plaquette belonging to A.F.L. Beeston who, according to his "laudemus invicem" partner, "avait trouvé cet objet dans sa documentation, sans pouvoir établir de qui il le tenait, ni si une provenance lui avait été mentionnée" (p. 1370).

(Follows p.81C)

That such an unusual inscribed antiquity would leave no trace in Beeston's advertised memory shows how little a beautiful original inscription actually means to an abstract theoretician. This remark is illustrated by what is presented by Beeston himself as "two amusing experiences" that occurred in Palestine in early 1941 (cf. his "autobiography" in Sayhadica, Paris, 1987, p. XVII), an example of vivid, accurate memory up to the last detail when his own ego is involved. Of course, an inscribed copper plaquette has hardly anything to do with Beeston's ego. Being a faithful member of the "laudemus invices," J. Ryckmans took Beeston's statement on his lack of memory as a full-stop sign and proof! However, as soon as I saw the photo, I knew that I had already seen it somewhere ... and within a few seconds I was looking at it in Kaiky Muncherjee Album N III, no. 19 (cf. my booklet Les albums photographiques de la collection Kaiky Muncherjee (Aden), Rome, 1955, pp. 65 and 67A). These photos were probably taken in 1923, and the albums made in early May 1925 (cf. l.c., p.3). I presume that Muncherjee might have given the plaquette to R.B. Serjeant who passed it on to Beeston; but the latter might have gotten it from Muncherjee himself. The preceding details might awaken Beeston's memory so as to enable him ... to belatedly shed some light on a possible missing link between Muncherjee and himself. - Cf. also below, p. 187.

Preliminary note on the two photostats.

The notes on hsṣtn, dqᶜ1tm and dmr(1.7) are labeled "(14)," "(17)" and "(18)" respectively; each number is seen in a margin and handwritten inside a small circle by someone different from the author of the notes. Those numbers presumably belong to the list of ScOA's suggested changes actually inserted in ScOAI.

I - The photostat of ScOA's revised p.65 on hsṣtn = ScOAI, p. 26.

This photostat deserves a special attention because hsṣtn was NOT the object of any remark in my 1994 text. Therefore, why did Müller choose that page preferably to any other listed in my 1994 study and, very especially, preferably to the photostat of ScOA's p.3 with the CRUCIAL testimony of the "Vorwort? The answer is self-evident. Nonetheless, the photostat of ScOA's revised p.65 gives the reader, who did certainly NOT need it, an extra proof of the lousy supervision provided by a biased Müller.

A - Indeed, Müller failed

1 - to correct the erroneous reference to F. Altheim's paper, "S. 130" to "S. 129-130;"
2 - to correct the bibliographical entry of F. Altheim (p.4) by not adding: "2nd edit., 1969; 6th chapter: Die Araber, pp. 124-157;"
3 - to acknowledge the existence of the 2nd half of the title of M. Rodinson's paper, ", royaume d'Imru l-qays" which is given in p.10, and no shorter title or abbreviation in p.13;
4 - to correct the ridiculous reference of "S. 114ff" when the whole reference is as simple as "S. 114-116," as in ScOAI, p. 10.

B - ScOAI gives the undeniable proof that those writings of F. Altheim and M. Rodinson, dating respectively of 1959 and 1954-1957, SHOULD have ALREADY been in 1982 ScOA; but they are not (cf. pp. 11 and 14, respectively). Their absence from ScOA AND their presence in ScOAI obviously are one example of what Müller's derailed lingo calls an "im wesentlichen unveränderter Druck" (cf. pp. 81A, E [twice], F, 84 and 89).

C - ScOA's commentary = ScOAI's 1st part do not even mention my in-

(Follows p.81D)



terpretation of hsṣtn as "reservoirs, tanks" (cf. Arabic ḥiṣâṣ/muṣâṣ, pl. ḥuṣṣat) as a reference to ᶜAden in my Sabaean Inscriptions from Mahram Bilqis (Mârib), Baltimore, 1962, pp. 71A and 318B, an opinion which I still maintain. Instead, as fully expected, ScOA = ScOAI (1st part) mention only H. von Wissmann's (Müller's good friend and benefactor) theory borrowed from others with reference to his book Zur Geschichte ..., Vienne, 1964, p. "404." This page is nothing but a short repeat of the study of pp. 186-187 and fn 385-387, and this is the correct reference to be used. von Wissmann's argument against my opinion reads as follows:

"die berühmten Tanks von ᶜAden. Diese wurden ... erst von den Persen errichtet und werden nie as Ḥaṣṣa bezeichnet" (cf. l.c., fn 387).

First, I have never suggested or hinted that hsṣtn would refer to the tanks preserved and still now visible. The Persians (cf. Western Arabia and the Red Sea, London, 1946, p.35) utilized the natural crevices, reservoirs, etc where the water gathered, to improve them and make the most of them. Secondly, the 2nd argument is valueless because, if it has any value, it should also work against von Wissmann's theory as the place called "al-Ḥuṣṣ" which he retains, has never been presentend as hsṣtn.

It should be emphasized that von Wissmann first speaks of "Ḥaṣṣa" but later identifies the place with "al-Ḥuṣṣ" without informing his reader of his switch of vowel; cf. Arabic ḥuṣṣ, pl. ᵓaḥṣâṣ or ḥiṣâṣ "a booth of reeds, or canes;" and ḥâṣṣ "particular, peculiar." We are still a long way from von Wissmann's "Ḥaṣṣa" which includes a tāᵓ marbûṭa. But, the letter ṯ of hsṣtn, no more than ṯ of "ḥaṣṣat" (cf. above, p. 81B, 1st line), is definitively not a tāᵓ marbûṭa. Cf. also my Sabaean Inscriptions ..., l.c., pl. 6 for the location of hsṣtn, mrb, and kdt = Kindat. And mrb Marab is well-known as the name of one 3rd of the ᶜAden population; cf. New Encyclopaedia of Islam, 1 (1960), p. 181A, and L. Forrer, Südarabien, Nendeln, 1966, pp. 42 and 213. This last testimony, H. von Wissmann, J. Ryckmans, M. Rodinson, etc prefer to carefully hide it from the reader.

Furthermore, what an anachronism to relate a text from the two Sabaean royal brothers of the 1st c. B.C. to the Laḫmids capital, al-Ḥîra founded at the beginning of the 4th c. A.D. by the father of ᵓImruᵓl-qays, who died in 328 A.D.! This anachronism would be somewhat reduced in the hypothesis of a more recent mini-chronology of the two Sabaean royal brothers. But, no proof of such a telescoping has ... yet been presented! Finally, how easy it is for anyone to mention "al-Ḥuṣṣ in der Nähe von al-Ḥîra; etwa 30 km südlich von Nagaf"! Why to hide from the reader the simple information that would be readily understandable, viz about 1870 km (as the crow flies) separate al-Ḥîra, south of an-Najaf, from Mârib almost straight south and slightly to the east? The coordinates of both places are as follows:

an-Najaf: 32° 07' N - 44° 07' E (cf. Gazetteer - Iraq, Washington, 1957, p.20B) and
Mârib: 15° 25' N - 45° 21' E (cf. Gazetteer - Yemen Arab Republic, Washington, 1976, p. 83B).

Cf. an additional note below, pp. 89-89A.

II - The photostat of ScOA's revised p. 72 on dqᶜ1tm = ScOAI, p. 29.

Several examples displayed below exhibit a Müller not caring for any serious study of a text provided his name appears in the commentary. The same yardstick is also applied in his utilization of the inscriptions, viz the mention of hrwm, the cultivated plain near wᶜln = the city of J. Masᶜâl with the fortress šhrrm, clearly indicated in Ja 2867/5, did not receive an entry in ScOA, p.150 = ScOAI, p. 59 ... probably because the text involved was not published by Müller! On šhrrm (cf. ScOA, p.91 = ScOAI, p.36), the reference to Ja 2861/4, ᶜrhw/šhrrm, also is missing. In the reference to Ja 2867, the mention of l.2 also is missing, and the omission is voluntary, as demonstrated below, under thrgb. ScOA's = ScOAI's statement on "W.W. Müller zu Recht ... Šiḥrān,

(Follows p.81E)

*beute Kis'ā" is erroneous (cf. already above). Even for al-Hamdānī, "Šiḥrār" is a pa-*
*lace" (cf. N.A. Faris, The Antiquities of South Arabia, Princeton, 1938, pp. 39–40 =*
*in N.A. Faris, الأكليل الجزء الثامن, Beirut-Ṣan'â', no date, p.53),*
*and neither a city nor a mountain. The name šḥrrm belongs to the nominal formation*
*gtlln that, obviously inexistent in Arabic, is firmly attested in South-Arabian; cf.,*
*f.i., ġ'rrmm in CaTh 31/1-2, hḫrmm in my paper in BASOR, No. 172, Dec. 1963, p.49,*
*hrddn of CIH 287/5, and l'ggm of Ja 631/26 (cf. my Miscellanées ..., XVII [1989], p.*
*85). Therefore, even al-Hamdānī's spelling "šiḥrār" may be the only known Arabic voca-*
*lisation/pronunciation, but it cannot be the genuine South-Arabian pronunciation since*
*the ending -m is missing from "šiḥrār." Another example of the distortion of a name due*
*to pan-classical Arabic fanaticism is dq'ltm of ScOAI, p.29, of which the 4 lines devo-*
*ted to the entry dq'ltm ARE NOT TO BE FOUND in ScOA, p. 72. THIS is the SECOND ADDI-*
*TION (one in each photostat!) to ScOA; cf. above, p. 81D for the first, of which my*
*remark must be repeated here:*

*The absence of those four lines forming a whole entry AND their presence in ScOAI*
*obviously are another example of what Müller's twisted lingo describes as an "im*
*wesentlichen unveränderter Druck."*

*According to ScOAI's handwritten note on dq'ltm of Ḫuqqa 26/3, the name contains "wahr-*
*scheinlich eine Metathese des arabischen 'qal'atun' in der Bedeutung Schloss."*

Both al-Scheiba and Müller apparently ignore the plain Arabic form qa'ilat, pl. qawā'ilu long mountain, the top of a mountain.

*Futhermore, Müller does not schrink from imposing his own idiomatic preference to al-*
*Scheiba's text: he replaces the author's "wahrscheinlich" by his own "vermutlich." No*
*explanation; no justification; no notification! No vindication at all!*

*Another Müller's interference is as follows. al-Scheiba speaks of "möglicherweise nicht*
weit von al-Ḫuqqa, dem Fundort der Inschrift."

*This original statement, too, is mistreated by Müller's dictatorial stylistic prefe-*
*rence, viz "möglicherweise" is simply wiped out without any compensation at all; and*
*the rest becomes: "deren Lage ..., die aber unweit von al-Ḫuqqa, dem Fundort der*
Inschrift."

*This is the third example: the presence AND the absence of "möglicherweise," which de-*
*termines the value of the statement, are for Müller 's disturbed mind an "im wesent-*
*lichen unveränderter Druck"!*

*Müller's third mishandling of al-Scheiba's original text is his substitution of his*
*pet word "Burg" instead of "Schloss."*

*The preceding examples make it reasonably impossible to still credit Müller with the*
*the basic respect due to the originality of his student's choice of words.*

(Follows p. 81F)



Finally, the accuracy of the information written in the Museum file is not reflected by either Y.M. Abdallah or al-Scheiba. Here is the text of my 1994 edition.

Ḫuqqa 26 is a four-line inscription incised in a limestone block found in the gate of al-Hajj ʿAlī Yaḥyá Ṣāliḥ's garden in Jāhilīyat, about 5.3 km east of Ḫuqqa. The text is by the 1st author of CIH 214 (cf. G. Garbini, in AION, 34 [1974], pp. 292-293 = A.H. Sharafaddīn, Taʾrīḫ al-yaman aṭ-ṭaqāfī, Cairo, II [1967], fig. 103, p. 104); it is published immediately below.

Ja 3256 = Ḫuqqa 26: "height: 31 cm; length: 53 cm; letters: 7.5 cm;" cf. Ṣanʿā' Museum file from which the abovesaid museographic details, including the siglum were copied:

1 drḥʾl/yhḍl/wʾḫhw/bnw/s
2 qrn/ʾdm/bn/btʿ/brʾw/byth
3 mw/bhgrhmw/dqʿltm/wrṯdw/by
4 thmw/ʿttr

1 Darahʾil Yahḍul and his brothers, descendants of (the clan) Sa-
2 qrān, servants of (the tribe) Bataʿ, have built their
3 house in their town Dūqaʿaltum. And they have entrusted their
4 house to ʿAttar's care.

yḍḍl, also in CIH 178/2. The name "yhḍb" of CIH 214 was corrected by G. Garbini (cf. l.c., p. 293) to "yhdl" that must be further amended to yhḍl. - ḥgrn, also, e.g. CIH 156. - bythmw (ll. 2-3 and 3-4) is called ybḍr (cf. photograph in G. Garbini, in AION, 30 [1970], pl. XXVIII b); cf. also Garbini, in l.c., 34 (1974), p. 293. - dqʿltm, cf. above, p. 81E.- Chronologically, this text precedes CIH 214 +.+ RÉS 3998 + CIH 199 +.+ RÉS 4001 (cf. G. Garbini, l.c.) where the author's two brothers are mentioned by name , and which adds another new precision, the name of the house.

After examining the two photostats sent by Müller, my findings are few, but there are only two short notes altogether. Those findings, however, are much more valuable that I had expected, and they remain unheralded and unjustified, viz

1- The author's choice of words ... replaced by Müller's synonyms ..... 2
2- The author's choice of an expression ... replaced by Müller's equivalence ... 1
3- The author's crucial adverb ... simply eliminated by Müller..... 1
4- The removal of that adverb and
5-6- the author's addition of two references ... are for Müller a mere triffle because the new text IS - in Müller's own words - "Im wesentlichen unveränderter Druck der Dissertation von 1982"... 3

The 3rd note calling for the insertion of a coma in a number (cf. ScOAI, p.29, dmr, l. 7) is meaningless because there is nothing to argue, nothing to discuss, nothing to choose and nothing to change: a coma is a coma. But, the 7 preceding examples testify

(Follows p.81G)

to an evenly selfish approach which, under the fallacious pretext of editing an original text (also upheld by some journals and institutions) tramples under foot a fundamental human right, the cornerstone of distributive justice, the unevadable "cuique suum." This loathsome attitude which never justifies any of its moves and of which the verdict is always final, is seen here in three different fields, viz

1 - the substitution of synonyms or an equivalent formula;
2 - the removal of an adverb affecting the value of the statement; and
3 - the cynical "im wesentlichen unveränderter Druck" proclaimed on contradictory facts, such as the absence and presence of a critical adverb and entries.

What a pity that Müller's stinginess made him to restrict to 2 the ScOA's 160 photostats which would have allowed to complete the preceding inquiry and to show all manipulations (including possible new types of abuse) and the specifics of each one! This complete inquiry, unfortunately, will never be done because, even were Müller willing to send all other photostats, his move, being a postmortem self-exculpation, would be significantly weakened by a much abated trustworthiness (cf. above, p. 81A).

The two authors' lack of judgment is further illustrated by this, viz a basically vicious attitude putting on the same level two publications of clearly contradictory quality. ScOA's pp.6-7 = ScOAI's p.2 claim that BOTH G. Ryckmans' Les noms propres ... (1934-1935; instead of "1934" only) and G.L. Harding's An Index ... (1971) are obsolete because of the numerous new texts published after them. Such an association is an insult to G. Ryckmans whose work remained valid for several decads and still is very useful for early texts and concordances. Quite on the contrary, Harding's volume was ALREADY INADMISSIBLE the very day of its publication by reason of the innumerable errors of all kinds scattered all over the book, as proven by the review (cf.below, p. 81I on A.K. Irvine) of my Miscellanées ..., II (October 8, 1971), pp. 93-150. This is another example proving al-Scheiba's and Müller's contempt of the unavoidable "cuique suum." Furthermore, neither al-Scheiba nor Müller nor the two of them have produced or could produce a work comparable, even far from it, with G. Ryckmans' epochal triptych.

It is now possible to go back to the 1994 text. Müller has actually approved so many fallacies or plain deceits the responsibility of which he cannot shrug off, that a whole booklet would be necessary to publish them all. Here follow a few remarks which will give the reader an idea of what he may expect from Müller's supervision.

1 - There are changes in ScOAI; f.i. those in the introductory note (cf. above, pp. 81-81A); "RC-ANL" of ScOA (p. 31) becomes "RCANL" of ScOAI, pp. 5 and 13); the order of CIASA and CIS is reversed (p. 5); etc.

2 - There are additions; f.i., the references to F. Altheim's 1959 book (cf.

(Follows p.81H)



above, p. 81C; to G. Garbini's paper in AICB,34 (1974), pp. 291-299 (p.5); the entries of "TSDD" and "DQʿLTM" respectively, pp. 19 and 29; for dqʿltm, cf. above, pp. 81D-F.

3 - There are deletions; f.i., the name dyfd and zrr of RÉS 4781/2 and 3 (cf. ScOA, pp. 74 and 80) are not listed in pp. 30 and 32 of ScOAI; note that the other name ʾbyn (1.5), is not followed by the reference to the text in ScOA, p. 35 = ScOAI, p. 14; the entry of ʾLM of ScOA, p.39 does not exist in ScOAI, p. 16.

4 - The special case of the siglum "YAMP" of ScOAI, p. 14, but not listed in ScOA, pp. 28 and 34.

a - "YAMP" is erroneous as it should be "YARM" since it is supposed to be the siglum of "Yemen Arab Republic, Map." Where does the sequence "MP" come from? What did the author have in mind? Who knows?

b - This "YAMP" has the same meaning as "MYAR" = "Map, Yemen Arab Republic" of both ScOA, p. 31 = ScOAI, p. 13. Thus, "MYAR" and erroneous "YAMP" are al-Scheiba-Müller's invention of a new example of "chou vert et vert chou." They did not try to imitate J. Pirenne's crazy system of the numbers of her Corpus des inscriptions..., where the numbers are equally read forward and backward (cf. also C.Robin below,p. 85).

c - Although neither "YAMP" nor its title are listed in ScOA, pp. 34 and 28, respectively, the siglum "YAMP" is mentioned in the same places of both ScOA and ScOAI THRICE more often than "MYAR," viz 27 times against 9, viz

"YAMP" in ScOA, pp. 50, 59, 66, 72, 76, 78 (twice), 79, 81, 87 (twice), 89, 93, 94, 95 (twice), 99, 101, 103, 104 (twice), 113, 115, 119, 120, 125 and 127.

"MYAR" in ScOA, pp. 38, 42, 131, 134, 145, 147 (twice), 148 and 155.

S u m m a r y :

| | | listed in | | used in |
|---|---|---|---|---|
| "MYAR" | siglum | ScOA, p. 31 | ScOAI, p. 13 | ScOA, 9 times |
| | title | p. 19 | p. 8 | |
| "YAMP" | siglum | NO, p. 34 | p. 14 | ScOAI, 27 times |
| | title | NO, p. 28 | NO, p. 11 | |

5 - A case of false identification. - ScOAI (p.5) still gives G. Garbini as the author of "Iscrizioni minee, Napoli" with the date of "(1974)," as ScOA,p. 14. Garbini's introduction of the book emphatically states that

"La preparazione dei singoli volumi è stata o verrà affidata a uno o più collaboratori. Questo primo volume, contenente le iscrizioni minee, è stato curato dalla dott.ssa A. Capuzzi" (p. X, ll. 6-8).

W.W. Müller does not even mention the authoress' name in his journalistic prose of AfO, 25 (1974-177), p. 504B; A.F.L. Beeston does the same in JSS, 20 (1975), pp. 266-270, but he speaks of the "editors"(!!!) in p. 269. C. Robin does the same in Biblio-

(Follows p.81I)

graphie générale systématique (Louvin, 1977), p. 131, no. 30.1974, a number which he ascribes to Capuzzi in p. 412B.

Furthermore, the date of "1974" printed on the front cover and p. III (unnumbered) of the book and repeated by ScOA, ScOAI, Müller, Beeston and Robin must be corrected to 1975. The book is listed for the 1st time among the "ultime pubblicazioni dell'Istituto" on the outside back cover of AION, 35, "fascicolo 2," 1975, where it is preceded by the last two entries of the back cover of AION, 35, "fascicolo 1," 1975. The year "1974" was the year when the composition of the book started.

6 - A case of ugly partisanship manifested in a voluntary omission of the relevant information. Cf. already above, p. 81G.

ScOA, p. 17 = ScOAI, p. 6 refer to A.K. Irvine's review of G.L. Harding, An Index ... published in JSS, 18 (1973), pp. 297-300, where the author ends his review with these words: "this book should still prove of immense value to scholars and provide a useful starting point, one hopes, for many profitable research projects in future."

It is hard to believe in the genuineness of the author's blindness.

But, my Miscellanées ..., II (1971), with its very long review is not even listed in ScOA's bibliography, p. 18 = ScOAI, p.7. And how could Müller make his reader believe that the "(sic)" after "ancient" does come from al-Scheiba, and not from himself?

7 - The case of the siglum "NKB" of ScOA, p. 31 = ScOAI, p. 13 defies any classification.

The siglum "NKB" is supposed to represent, "Nāmī, H. Y., Nuqūš Haribat Barāqiš," The capital "K" comes from the habit of some authors of transcribing Arabic hâʾ as "kh." But, al-Scheiba NEVER does it in his transliteration of Arabic titles; f.i., in his entries on Nâmî, the reader finds 4 times the letter "H" (p.31, and thrice in pp. 21-22 = ScOAI, pp.13 and 9, respectively), and in ScOA, p.16 (last line) = ScOAI, p.6;
p.17 (1st line) = ScOAI, p.6;
p.22 (ll.1,2,7 [twice],12) = ScOAI, pp.8-9); and
p.26 (l.10) = ScOAI, p.10.

Thus, the siglum "NKB" exhibits two inconsistencies, "K" instead of "Kh," and "K" instead of "H."

8 - ScOAI (p. 13) introduced the error of "People's" in the entry of Southern Yemen instead of the correct "Peoples" of ScOA, p. 31.

9 - ScOAI failed to complement many entries of ScOA's bibliography, where the presence of plates accompanying a literary production is never mentioned; f.i., the mention of vol. III of A. Fakhry's publication (p.5) is still missing!

(Follows p.82)



10 - ScOAI failed to correct some of ScOA's errors; f.i., "šarḫihâ" (cf. also C. Robin in Bibliographie générale ..., p. 133) in H.Y. Nâmî's 1st entry (p.8; already ScOA, p.21) instead of šarḫuhâ; and ScOA's error of "Arabia" to "Arabie" in G. Ryckmans' entry (p.31) and ScOAI, p. 13.

11 - The original map of ScOA, "Taf. I," is illegible; it became pl. I where the delineations of the two squares corresponding to maps II and III are still maintained; they are all the more meaningless that no number indicates which square is II or III. ScOAI failed to change the 39 ScOA's references to "Taf. II" and "Taf. III" (cf., f.i., brʾn and bʾsn of p.17) to "Taf. I." At the same time, he failed to correct the three ScOA's erroneous references to "Taf. III" under tmnʿ (p.20), gwʾl (p.22) and ḫzym (p.24); he also failed to add to bynn (p.18) the reference to the plate, viz in pl. III of ScOA, but pl. I of ScOAI. The advantage of the reedition is that ScOA's illegible map I is legible in ScOAI. This map, however, was NOT checked by ScOAI. The characteristic of ScOA's maps is that a fancy form of the capital N, ⋃ (cf., f.i., NʿMN = NʿMN), may sometimes be read H; and this error happened at least once in the case of ʾknṭ = "ʾKNṬ" of ScOA's pl. II, and
= "ʾKHṬ" of ScOAI's plate I! The angular ≥ points to the same worker.

12 - ScOAI's "DLG" (p. 27). - In l.2, he added to the original text the reference which should have already been in ScOA, of RoBaMG 1/11: "dlg" (cf. Raydān, 3 [1980], p. 86 and photo of pl. 85). If this edition prompted ScOAI to adapt the original parenthesis by changing his "sabāisch" to "(beide sabāisch)," he failed to utilize the text in his argumentation on Ja 576/4, the conclusion of which repeats that of M. Höfner (endorsed by C. Robin and M. Bâfaqîh in Raydān, l.c., p. 90) but which is introduced by an affected pronunciamento: "halte ich dlg für die richtige Lessung dieses Stadnamens." ScOAI refers to M. Höfner's 1981 booklet, but ignores my Miscellanées ..., X (1979), which has NO entry in the bibliography of ScOA, p.19 = ScOAI, p.7. Therefore, his claim about "Jamme (ohne Foto)" is a voluntary falsehood. The photo published in my Miscellanées ..., l.c., clearly shows dll. It suffices to compare dll with the two mentions of hgr in the same l.4. The preceding remark was already published in The Wadi al-Jubah Archaeological Project, Washington, I (1984), p. 71. But, this publication is also conspicuously absent from the bibliography of ScOAI. As it seems natural to him, ScOAI affectedly and willingly ignores the published testimony against his thesis. Finally, here also, ScOAI maintains the reference to "Taf. II" of ScOA, viz to something that does not even exist.

13 - ScOA flatters Müller; cf., f.i., the commentary on RÉS 3550/4 (cf. "ḤKB" of p. 58 = ScOAI, p. 23) is unacceptable because it shows a strong dependence on N. Rhodokanakis as well as a confusion of the realities involved; cf. my Miscella-

*Bées ..., III (1972), pp. 57-58. Müller is, as usual, so infatuated by his own contemplation that he did not correct the erroneous "WBG" to "WGS" at the end of the commentary. The 11 lines of p.34 of ScOAI devoted to the ancient city ms(h)rm, identical with the 19 of ScOA (pp. 85-86) do not even mention the excavator's final report of his work, viz F.P. Albright, "Sumhuram" in my Miscellanées ..., XI (1980), pp.1-66 and pl. 8-11, and especially The American Archaeological Expedition in Dhofar, Oman, 1952-1953, Washington, 1982, which supersedes all previous work, including Müller's prose a phrase of which is ... naturally ... quoted, and another referred to. These two works are not even listed in the bibliography of ScOA, pp. 19 and 11 = ScOAI, pp. 7 and 4, respectively. In an effort to make up for the elimination of the essential material, al-Scheiba and Müller give as their first reference "WZA 32" = H. von Wissmann, Zur Archäologie und antiken Geographie von Südarabien, Istanbul, 1968, p. 32, whose contribution is incomparably exceptional in importance, viz "einen anderen Tempel in SMHRM (Bür Hūrī)" without any footnote!!! Müller was so elated at seeing his work explicitely referred to that he paid no attention to the rest and, consequently, he failed to correct the following errors.*

*a - There is a duplication in the bibliography, viz "WBG 195" (1.8) and "WBG 195, Anm. a" (1.11). The reference, however, is erroneous: "a" should be corrected to "o." Incidentally, this reference is of vital importance: it informs the reader that F.P. Albright's initial sketch of smhrm may be found in W. Phillips, Qataban and Sheba. But, the page number is not given; that sketch exists already in each of the two F.P. Albright's papers listed in their bibliography; much more importantly, that initial sketch was considerably improved in F.P. Albright's final report which is ignored in both texts.*

*b - At the end of the mention of F.P. Albright's 2nd paper, "38f." must be corrected to "37ff."*

*c - At the end of the mention of J. Pirenne's paper, "91" must be corrected to "81."*

*14 - Under "ḥqlm" (p. 24; cf. ScOA, p.59), the reading of ḥql/ḏrmtm is declared "von A. Jamme sicherlich falsch zu ḥql/ḏrmtm ergänzt" in Ja 590/10-11" ... without a single word of explanation! al-Scheiba's statement, whether strongly phrased or not, is valueless since it only spells out the unsubstantiated claim of his master. In the two authors' reference to "Ja 2107/11," correct "11" to "10." It is very simplistic and quite easy to imagine that the expression bḥql/ḏrmtm of Ja 578/7-8, 34 and 2107/9-10 is so close to bḥql/ḏrmtm with a damaged ḏ, a fact that has been noticed and noted long ago, that it demands the reading of the first to be found in the second. A.F.L. Beeston's rendering (?) of Ja 590 (cf. Corpus des inscriptions et antiquités sud-arabes, Paris, II [1986], pp. 13-16) is useless because it does not at all improve upon my Sabaean Inscriptions from Maḥram Bilqīs, Baltimore, 1962, pp. 96B-97A; all that is seen is Beeston's normal illogical fanciness. In his first note (p.14) according to which the name s'dnm is said to be "visible" in my Sabaean Inscriptions ...'s pl.7 (cf. also his "description," p.13), although the two extreme letters only remain in Corpus des Inscriptions ...'s photo (p.15), Beeston nonetheless transcribes "s'(dn)m" (p.13); thus, indicating that, for him, the three median letters do not exist and are restored. But, thanks to some magic hoepoe, the name becomes "s'dnm" in the translation(p.14)! Contrarily, ʾlmqh (1.2) is visible in my Sabaean Inscriptions ...'s pl. 7, but only "l..." in Corpus des inscriptions ...'s photo, Beeston gives the name as "ʾlmq[h]" in the transcription of p.13, but, here again and thanks to the same hoepoe, as "ʾlmqm" in the translation of p.14! Faced with such obvious examples of crass illogicality, the reader can only turn the page ... only to find more of the same.*

*Beeston's 2nd note (p.16) deals with "(d)rmtm" which he records as "ḏrmtm" (p.13) without any explanation of the special location of * that elsewhere appears above the letters (f.i., p.6, in nine letters). Beston's genial justification of his reading is as follows: "the two upper members of the disputed letter are too far apart, even as seen in his photograph, for this reading [= that of drmtm] ... all that has happened is that the middle prong of the letter has become effaced" (p.16).*

*I find it impossible to understand how an author can write two statements so viciously erroneous because they are so manifestly contradicted by the facts for anyone who is willing to see. Beeston's 1st sentence, "the two upper ... apart" do NOT exist: his 1st member is the word divider; his 2nd member "far apart" simply does not exist; its place was destroyed even when I copied the text. Finally, on his statement about "the middle prong ... become effaced," the upper section is still on the stone to the left of the word divider. It is most obvious that there is no room for a large ḥ between the word divider and rm. The position taken by al-Scheiba, Müller and Beeston is all the more shameful that none of them ever saw the original stone, and I studied it before it was further damaged; my copy from the original shows the letter ḏ, as given in my Sabaean Inscription ..., p.96B; cf. also the photo of Ja 590 in JRAS, 1968, pl. IIc (left center), facing p.13, a photo that none of those argumentative babblers refers to.*

*Finally, ScOAI adds to ScOA a reference that should have already been given in the original study, viz "BR-M. Bayḥān 1/10;" same siglum as, f.i., in Raydān, 3 (1980) 1981, pp. 84foll, another proof of Müller's big lie on "in wesentlichen unveränderter Druck" (cf. above, p. 81A). The 2nd element of that siglum stands for "Musée de Bayḥān" (cf. l.c., p. 83), an appellation also used by F. Bron in Sayhadica, 1987, p.22. In both cases, "the reader wonders why the official siglum in Arabic or in English (cf. NAM = National Aden Museum) is not mentioned between parentheses or in a footnote" (cf.*

*ay Miscellanées ..., XVI [1988], p.82, and pp. 81-92 on Bron's paper). The 1st element of the siglum is incomprehensible unless the order of the letters is reversed; viz "RB" = Robin - Bāfaqîh, the two coauthors of the paper in Raydān, l.c. Note also that the unique siglum "BR" (p.12) stands for the two enumerations "Robin - Bāfaqih" and "Bāfaqih - Robin" probably in imitation of the "chou vert et vert chou" of the sigla of Corpus des inscriptions ..., I and II, where every siglum may be read in either directions; cf. also above, p. 81E for another example of that nominal juggling of the al-Scheiba-Müller tandem.*

*The name "Bāfaqih" of ScOA, pp. 11, 29 and ScOAI, pp. 4 and 12 must be corrected to "Bāfaqîh," as in ScOAI, p. 10; and his initials are "M.A." or "M.ᶜA." as in ScOA, p. 10 = ScOAI, p.4.*

*In their commentary on bḫqln/dhrmtm (l.10), Robin and Bāfaqîh claim that the reading "ḫql Ḥrwtᴹ" must be read in Ja 590/10-11 (cf. l.c., p.89), but the reader is not given any reason for their recitation. The two coauthors also claim that South-Arabian ḫql "correspond très certainement à l'arabe yéménite ḥaql" with reference to two cases of al-Hamdānī's Sifat jazīrat al-ᶜarab (cf. L. Forrer, Südarabien, Nendeln, 1966, p.187). The expression "l'arabe yéménite" is erroneous; the noun ḥaql is well-known in Arabic and cannot be considered as specific to Yemeni Arabic; f.i., the word is wanting in the study of I. al-Selwi, Yemenitische Wörter in den Werken von al-Hamdānī und Naŝwān und ihre Parallelen in den semitischen Sprachen, Berlin, 1987, p.71. Even more importantly, it seems obvious that Yemeni Arabic has retained only for ḫql (cf. "ḫjil (!)" and "ḥajlah" of M. Piamenta, Dictionary of Post-Classical Yemeni Arabic, Leiden, 1990, p. 101B) the 2nd meaning of classical Arabic (cf. E.W. Lane, An Arabic-English Lexicon, p. 612B, ḫaql). Finally, none of those five above-mentioned babblers, viz al-Scheiba, Bāfaqîh, Beeston, Müller and Robin, ever refers to the fact that H. von Wissmann had already stated in 1964, although without a single word of justification (cf. Zur Geschichte und Landeskunde von Alt-Südarabien, Vienne, p. 397) that the expression "ḫql [Ḥ]RMTᴹ" was already attested in Ja 590.*

15 - The 2nd half of the comment on thrgb (p. 20 = ScOA, p. 51) repeats - of course - Müller's concoction according to which thrgb is an epitheton of the city of Mârib (cf. Raydān, 3 [1980], pp. 64-66), and which mixes gratuitous and wishful thinkings.

a - Müller begins by using his imperceptive pan-classical Arabic fanaticism to posit that my vocalization of thrgb as "Tahargab" is "mit einer kaum gerechtfertigten abweichenden Vocalisation des Namens" (p.64). That blinding fanaticism prevents him from comprehending that [a] Arabic is not South-Arabian; and



[b] the South-Arabian vocalization system remains unknown. It is, therefore, foolishly quite pedantic to impose South-Arabian any vocalization of classical Arabic, as he already vocalized šḥrrm as "Šiḥrār" (cf. above, p.812). Furthermore, Müller repeatedly vocalizes it as "Tuhargib" in his translation of the pericope (cf. also in O. Kaiser, Texte aus der Umwelt des Alten Testaments, Bd.1, Gütersloh, 1985, p.662, and especially note d, at the end of which "63f" should be corrected to "64ff"). Since he considers the word to be a passive verbal form, viz "welche verehrt wird" (the relative pronoun instead of "she" is an unjustified restoration as thrgb is an isolated name), his erroneous "Tuhargib" must be corrected to "Tuhargab." But, Müller even sees no difference between active and passive voices since he writes "Tuhargib (bzw. Tuhargab)" (p.64).

b - Müller considers thrgb as an epitheton of the city of Mârib, but - here again - without any reason supporting his suggestion, first, as "wahrscheinlich" in Raydān, l.c., p.66; then, as "möglicherweise" in O. Kaiser, l.c., fn d. The words "probable" and "possible" are thus synonyms in Müller's jargon, and even those two important restrictions disappear in ScOA! Therefore, "probable," "possible" and sure are synonyms in the al-Scheiba-Müller's lingo! This indicates that, for both of them, the first two "probable" and "possible" were, even from the start, mere tricks or window-dressings aimed at deceiving the reader. Contrary to those two pan-classical arabic fanatics, W.W. Müller and C. Robin (cf. Le pays de Hamdān et de Ḥawlān Quḍāᶜa (Nord-Yémen) avant l'Islam, Paris, 1977, p.198), the thqtl nominal derivation is not necessarily to be understood as the 3rd feminine person of the ḥqtl formation - this is their opinion since neither mention an alternative interpretation - because of the existing yhqtl form. The prosthetic t- is also known as the sign of the maṣdar. Furthermore, Müller's thesis does not make any sense at all because, according to him, the infantrymen would have been dispatched ... to Mârib from where they originate although the war was down south-southeast, between Sabaʾ and Qatabān.

c - Müller declares not to know of any other place name belonging to the formation thqtl. This nominal derivation indeed is very rare, but another example is the family name thtkn of Ja 1076 (cf. RSO, 40 [1965], pp.48-49 and pl.5, no. 14) and RÉS 3566/31 in RÉS, VI (1933), pp. 215 and 219: "Tahthakān;" for the vocalization, cf. also Le Muséon, 62 (1949), p.73, and 64 (1951), p. 122: "Tahtakān;" the name is also listed in RÉS, VIII Tables et index des tomes V, VI, VII (1968), p. 250B. The root tkn is attested in Safaitic, cf. tkn in CIS 4808, and tknt in CIS 2216, as well as in Thamudic ʾltkn in Hu 557a.

A case can be made of the Qatabanian masculine name thnʾm of RÉS 4942, related by RÉS

VII, p.444) to *hn²* on the basis of "*liḥ thn²mnyt* ((correct "y" to "w")) with reference to Libyanite JsaL 319 in G. Ryckmans, *Les noms propres*..., Louvain, I (1934), p.224A. However, *my Miscellanées* ..., VII (1974), pp.125-126 identifies JsaL 264 = 319 with the family name *hn²mwt*. *thn²m* can be understood as a *thqtl* derivation; cf. the masculine personal name *n²m Nā²im* in Ḥaḍrami Wissmann 10 d, instead of "*²nsm*" of G. Ryckmans in *Le Muséon*, 75 (1962), p.227: contrary to H. von Wissmann's facsimile, the original contains 4 names which I have seen and read on Saturday, December 23, 1961.

Müller would make his case even worse were he to reject the value of *thtktn* and/or *thn²m* because they are not place names. The nominal derivations of place names follow the same patterns as those of all other names, and do NOT constitute a special corpus of nominal forms. Finally, Müller also is wrong when translating a proper name according to its etymology instead of transliterating it (cf. also below, p.126). I wonder whether Müller would appreciate his name be translated on the basis of its etymology instead of being repeated as such. - **Cf. an additional note below in p. 89A.**

d - Müller refers the epithet of a city name to "*hgrn/wᶜln/t'zl* ... Inschriften am Fels von Misᶜal"(p.65). Müller voluntarily shows his ugly partisanship because that expression was published for the first time in Ja 2867/5, along with two other names + epithets, viz *šmsm/ᶜlyt* and *šḥrrm/mšmsm* in l.2; cf. their commentary in my *Carnegie Museum 1974-1975 Yemen Expedition*, Pittsburgh, 1976, p.118. The second point that Müller hides from the reader is that the expression *hgrn/wᶜln/t²zl* is NOT paraleled by any other expression, such as "*hgrn/mrb/thrgb*" which is INEXISTENT; what does exist is *hgrn/thrgb* only, and nothing else (cf. above, p.86).

e - Müller states that another example of an epithet apposed to a city name is *hywn/thrḫb* of RoPH 63/3 (cf. C. Robin, *Le pays de Ḥamdān* ..., pp.195-198, without any illustration of any kind!!!). As already emphasized in the edition, the fragmentary condition of the text does not allow any ascertained interpretation of the preceding expression. Of course, Müller does not need any context to make up his mind; he evidently knows ... as the self-proclaimed Great Python should ... instinctively.

f - Müller's new translation of the whole pericope containing *thrgb* (p.66,and in O. Kaiser, *l.c.*) is typically "Müllerlike" vitiated, viz

(1) two verbs in the singular, *hwrd* and *hrs*, are given each a plural object;

(2) the dual form *hrfy* is translated as a plural;

(3) the compound preposition *bᶜly* found in two phrases with parallel meanings is translated "over" and "for account of;"

(4) *hrs* is translated "to do military service" (cf. also *Sabaic Dictionary* ..., Louvain-la-Neuve, 1982, p.118) on the basis of the root *rss*. Cf. Arabic *rassa* "to dig



(a well) ... To bury, inter" (cf. H.A. Salmoné, *An Advanced Learner's Arabic-English Dictionary*, Beirut, 1972, p.278A, and G.W. Freytag, *Lexicon arabico-latinum*, Halis,II [1833], p.146B). It takes more than a disrupted mind to justify the preceding translation on the quoted meanings of Arabic *rassa*;

(5) his understanding of "*kbtn*" as the name of a territory located probably between *Mārib* and *Ṣirwāḥ* remains unjustified.It comes from the information given by RES 3951/2 where *kbtn* is a place name. But, Müller's interpretation is contradicted by the context which speaks of a war between Saba² and Qatabān in the south-southeast, but *Ṣirwāḥ* lies in the west. Furthermore, when discarding the meaning of *kbt* as *hostilities* (cf. already K. Conti Rossini, *Chrestomathia arabica meridionalis* ..., Rome,1931, p.167B, and my *Sabaean Inscriptions* ..., p.438A), *Sabaic Dictionary* ... did not know what to do with it: "?troops ≠ n.pr.? ?√?" = unknown root (p.76). That did not prevent A.F.L. Beeston, the primary contriver of that dictionary and its main driving force, from translating the word as "hostilities" in YM 367/6 in *Corpus des inscriptions*..., I, p.203. What a trickster, a turncoat in the same cynical manner as his two volte-faces already denounced in my *Miscellanées* ..., XIV (1985), pp. 80-82 and 90-93! Müller eliminates all reference to actual fightings. Then, it is unintelligible that *²Ilumquh* be praised for having *hwfy* = saved all those men ... who were NOT EVEN in the fightings!

**The pages already written in 1994** illustrate some aspects of W.W. Müller's loathsome working method that had already been summarily denounced in my *Miscellanées* ..., XIV (1985), pp. 132-175. Many other amazing examples of the same extravagant scheme are scattered all over this publication. One may, however, single out as especially revealing not only Müller's **wretched flattery of** F. Løkkegaard in the question of the sword Nationalmuseet 14646, denounced above (pp.54 foll.), but also his fallacious study of several ScS texts (cf. below), and especially of ScS 6 which is "worthy of being among the most valuable crown jewels of his favorite Queen of Sheba" (cf. below, pp. 112-113).

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When I accepted the two photostats sent by Müller (cf. above, p. 81B), I did obviously not know what I was about to read. What I discovered exceeds by far all my concepts, viz the shocking blueprint by Müller himself, of a selfish attitude having taken over the realm of the most fundamental human right indelibly branded in the most basic equity of the human society, the all-prowerful, the self-evident adage "cuique suum" (cf. already above, p. 81F).

The information contained in the previous pages, although far

from being systematic, is already so outrageous because of its depth and scale, and yet so obvious to anyone who is willing to read, that it brings up the fundamental question: what kind of mind would do such things? The answer is purely platonic because, regardless of whether or not such a brain is seen as pathologically unbalanced, misguided or blinded, the narratives already stigmatize once and for all its author on two accounts.

Not only did Müller endorse as a Ph.D. thesis a manuscript riddled with errors, inaccuracies, inconsistencies and partisan attitudes, which are so apparent that Müller could NOT possibly NOT have read them. But, Müller NEVER said to have read them all. Did he? He also applied the same extraordinarily exceptional attitude exclusively to the suggested changes of the revision leaving the rest untouched because - no doubt - it had already been approved by him previously!

And yet, although all those blunders are his own doing of which he indeed bears the whole, unmitigated responsibility - What else could al-Scheiba do except to go along with Müller's moods if he wanted his Ph.D.? - all that Müller thinks of doing and does is to indulge both in rehashing his snivels, his jeremiads and his biased and erroneous pronunciamentos, and in seeking a vainglorious sympathy through the repeated sentimental display of his bruised and bleeding ego. Such a childish behavior is a reminder of the story of a politician who was bewailing to Harry Truman the public rebuke of his own sayings by some peer. And Truman made a lightning retort: "If you can't stand the heat, get out of the kitchen, and thanks for the riddance." The last third of the quotation, rarely heard, is probably spurious.

How could there be a more simple and more radical solution to eliminate all recriminatory lamentation? Such a phrasaeology, however, might well be too logical to reach Müller. A more appropriate and more personal reason is fortunately suggested above. In fact, nothing could be easier for Müller than Truman's solution for the simple reason that writing and lamenting OR not, either one actually is in Müller's own words an "Im wesentlichen unveränderter Druck" (cf. already above, pp. 81 A, E, F, 84) of the other, viz yes OR no, no OR yes, it's all the same!

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Additional note to be added to p. 81D, at the end of the comment on hsstn.

This note exemplifies one main characteristic of ScOA = ScOAI, viz both al-Scheiba and Müller's crass partisanship (cf. already above, p. 83).

The entry on hsstn in ScOA (p. 65 = ScOAI, p. 26) is immediately followed by that on hlb of Ja 616/27. This context mentions a very unusual pericope, viz the list of 3 wadis

(Follows p. 89A)



located in a country northwest of Yemen and identified in the historical commentary of the publication of the inscription. The expression reads as follows:

-/ʾwdytn (27) dbrʾn/whlb/wtdhn - the wādīs (27) of Biʾrān and Ḫulab and Tadḥān

(cf. my Sabaean Inscriptions ..., 1962, pp. 114-115A) My commentary correctly identified the three of them, respectively, as Biʾrān, Ḫulab and Tindaḥat (cf. l.c., p. 340B and fn 178-183). al-Scheiba's and Müller's sense of honesty and respect of the basic principle of distributive justice is so vivid and saturates their work so much that neither make any reference to my identification of any of 3 wadis, viz

| | | | |
|---|---|---|---|
| bʾrn | in ScOA, | p. 42 = | ScOAI, p. 17; |
| hlb | in | p. 65 = | p. 26; and |
| tdhn | in | p. 47 = | p. 19. |

But, obviously enough, they do not miss any reference to H. von Wissmann, Müller's good friend and benefactor (cf. already above, p. 81D).

Under bʾrn, reference is made to "WZG passim, s. Register S. 452, WÖḪ 937."
In p.452, mention only of the general location and Ja 616/27.
The 2nd reference mentions "al-Bār" in Ja 616/27.

Under hlb, reference is made to "WZG 190, Anm. 389, WÖḪ 933."
The 1st reference is erroneous: "p.389" is in p. 187 and about W. ad-Dawāsir. - If it is fn "398" (instead of "389") of p. 190, the text deals with "Sanḥān."
The 2nd reference does not even mention Ja 616/27.

Under tdhn, reference is made "WÖḪ 937" which mentions "Wadi Tindaḥa (tdhn)."

The two references on hlb are erroneous; the 3 others mean nothing because they copied what had been printed before.

That's the way al-Scheiba with the blessing of the Grand Python Müller builds up references!

A final note concerns d of dbʾrn, which was rendered as "of" in the edition (cf. above) and followed by both ScOA and ScOAI. It is most obvious that d is not the sign of annexation as ʾwdytn is in the absolute state; thus, "of" is strictly idiomatic, and d which recalls wdy, introduces the enumeration of ʾwdyt.

This triple example of Ja 616/26-27 is another fitting presentation of the work of a master followed by his servant, who are publicly denounced for bending scientific research to their own selfish purpose and trampling under foot to their own advantage the fundamental principle of distributive justice, the "cuique suum."

The two photostats sent by Müller throw some light on his "supervision" or "patronate" of (onomastical) theses which seem to follow a unique pattern that might be sketchily summarized as follows: after presenting his own onomastical or historical views based on his pan-classical Arabic fanaticism (a personal conviction or a social artifice?) and the biased theories and partisanship of his "beni-oui-oui" club, Müller apparently limits his actual supervision exclusively, or almost exclusively, to stylistic matters, and this enables him to fashion his student's opinion to his own liking.

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Additional note to p. 87.

Noteworthy to point out is that the 3 texts with thqtl names are all Qatabanian. This information on dialectical onomastics suggests that the city thrgb also is Qatabanian.

V - Turner 1:

*Jacques Ryckmans' homage to O. Löfgren.*

A - Introduction.

Jacques Ryckmans' paper entitled "Une table à libations avec inscription sabéenne provenant du Ğauf du Yémen" dans U. Ehrensvärd and C. Toll edit., On Both Sides of al-Mandab, Stockholm, 1989, pp. 69-81, is published in homage to O. Löfgren. The following remarks do not result from a systematic analysis of the paper; it would take too many pages and waste too much precious time to correct all tendencious, dubious and plain erroneous statements devised by Jacques Ryckmans' well-known confuse and confusing method and style. The three following examples are aimed at placing the reader in the right athmosphere, if he is not yet accustomed to Jacques Ryckmans' production, viz no statement can be taken for granted, and every single one has to be checked for accuracy because it most often brings unsuspected surprises.

1 - NAM 1308 is referred to in both fn 8 (p. 71) and p. 73 to "n. 3" = fn 3 (p. 70). This twice-repeated reference is erroneous; there is no mention of NAM 1308 there. But, the reader finds that at the end of fn 5 (p. 70) the mention of NAM 1549, where the reference to the edition, erroneous for NAM 1549, is correct for NAM 1308. Yet, inasmuch as I understand Jacques Ryckmans' cryptic lingo, NAM 1549 is exact in fn 5 (p. 70), where "n° 2" should be corrected to "n° 1." Furthermore, NAM 1308 is listed with other antiquities in fn 8 (p. 71). This is contradicted by p. 73, where it becomes a "unique" specimen which is erroneously described as having horns and ears which "se dressent en oblique au-dessus de la surface dans laquelle est creusée la rigole" (italics in the original). A simple look at the photo shows that those horns and ears rise below = "au-dessous" (not "au-dessus") of the upper surface of the bucranium, neck and slab: viz above and below are all the same for Ryckmans.

Before turning to the next point, let us take up the last reference of fn 5 (p. 70) which reads as follows: "CIAS II, 2, p. II. 261-262, no. 2."

This kind of reference shows Jacques Ryckmans' servility of blindly copying the page identification as printed in the book because he uses the same senseless reference twice in the same page 70, one with the feature "II." (as above) in fn 4, but once with "II," in l. 11. This is, of course, an earth-shaking difference. Indeed, his repetition of "II, 2, p. II. 261" is senseless because "II.261" obviously cannot but mean



"II, 2, p. 261" since the 1st page of fasc. 2 is 230. The sigla "I.1" to "I.610" and "II.1" to "II.454" are justified in I, p. XVII as follows: "Il faut une reliure qui ... admette l'intercalation des documents nouveaux, ou les corrections à ce qui est déjà publié." Therefore, in the hypothesis that someone would unify the two volumes, "39.11/0 6 | n° 6" of "II.83-85" would take its places after "39.11/0 6 | n° 5" of "I. 83-85," and the numbers "I." and "II." would indicate the volume of publication. Similarly, "R 41 // 95.11" of II.351-352 to "R 47 // 95.11" of II 359-360 would take its places before "R 51 // 98.Fr" of I.457. Incidentally, this fasc. 2 begins with an outstanding example of J. Pirenne's pseudo-scientific workmanship, viz, pp. 235-238 are her dreamy, incomplete study of RÉS 3902, no. 120 (to be corrected to 120A) with reference (p. 238) to pp. 199-201, the fragmentary inscription. In neither places does Pirenne refer to my Miscellanées ..., II , published eleven years earlier, in 1971, p. 139, with the publication of RÉS 3902, no. 120B +.+RÉS 4155. This is an outstanding example of Pirenne's method. For my rebuttal of Pirenne's literature, cf. my Miscellanées..., XVI (1988), pp. 25-31 with the photographic reproduction of p. 139 of my original study.

2 - CaTh 7 (cf. G. Caton Thompson, The Tombs..., pp. 24, 160-161 and photographs in pl. XVIII, no. 3) is described by Jacques Ryckmans as having a horizontal line of "denticules" (p. 71). This is erroneous because there are only short, superficial, vertical, purely ornamentative lines; there are no dentils either above the eyes or in the center of the forehead. Furthermore, Jacques Ryckmans speaks of "un trait undulant annonçant les naseaux." There are two broken straight lines making each an obtuse angle representing the upper fold of the nostril.

a - Those ornamentative lines are also found across the top of the forehead on a fragmentary spout in J.-F. Breton et al., Le Wadi Hadramawt. Prospections 1978-1979, Beyrouth, 1982, pp. 44-45 and photograph in "Matériel" pl.III, top. Two additional notes on this pamphlet. (1) This booklet is listed by Jacques Ryckmans on his p. 79 as printed in "Aden, s.d., [1980?]." On p. 54 of the booklet itself, the reader finds this: "Impression: Mantoura. Beyrouth - Liban [.] Achevé 1er Trimestre 1982." Furthermore (2), the 44 plates are surprisingly enough distributed into 7 series, each of them having its own numerotation. Therefore, "Pl.III," as in Jacques Ryckmans' p. 71, is so precise that it may refer to any of the seven series.

b - The editor of CaTh 7, G. Ryckmans (cf. G. Caton Thompson, l. c., p. 161) reads the name of the author's father as ʾlqtm by incorporating an isolated q engraved on the right-hand side of the gargoyle, but the commentary adds that the name is ʾltm "ʾIltamma?" (sic!) "en excluant le q." The name itself is engraved

on the front of the libation slab and the perfect symmetry of the two sections, ʾl and tm (cf. l.c., photograph XVIII, no. 3, top) indicates that q stands alone as the initial letter of the engraver's name; thus, the name reads ʾltm ʾAltam. There is no reason at all to understand the name as theophorous. Cf. the Thamudic verb ltm to break the front teeth in But 545; and the names ltmt in both Safaitic, e.g., CIS 1501, and Thamudic JsaT 128; and Thamudic ltmdd of Hu 801. In order to be able to compare the tracing of that q (diameter of the circle and the letter height) with the palaeography of the main text, I requested some photographs from the Fitzwilliam Museum of Cambridge to which Caton Thompson refers explicitely (p. 160). A letter from the Museum dated of May 13, 1992, informed me that "there is no evidence" of the slab in the museum! G. Ryckmans does not even mention the important question of palaeography!

3 - Ḥôr Rôrî, the photograph of which is published by R.L. Cleveland in BASOR, No. 159, October 1950, p. 19, is described by Jacques Ryckmans as a libation table with a "longue mèche striée" (cf. fn 8, p. 71). This is obviously another example of Jacques Ryckmans' unchecked fancy, extravagant whimsicality.

a - A round hole is visible on the top of the front rim of the table, a few centimeters behind the junction of the bucranium neck and the slab proper. It is NOT connected with the hollow central cavity of the upper side of the slab, and cannot thus be a drain. In fact, Cleveland does not mention any exit on the bucranium which remains, as he says, "without a groove (spout) over the head" (p. 18). Therefore, this antiquity is a simple votive slab, and cannot be misrepresented as a libation table. b- With regard to the so-called "longue mèche striée," that becomes on the following page (p. 72) "une mèche asymétrique se terminant en pointe," it actually is a line representing a biconvex lens without a little more than its upper third missing; this design no doubt represents the bridge of the nose.

B - The antiquity itself.

1 - Jacques Ryckmans does not even specify the material the antiquity is made of. The question is not even raised nor the word itself alluded to in the paper, which is apparently intended to corroborate "à suffisance" (p. 73) his thesis of the bull being a symbol of fecundity because, as he puts it, the gargoyle representing bucrania and made to drain off the rain were modeled after libations table! What an empty gargle! Those tables are votive objects and, as such, have nothing whatsoever to do with rain. Jacques Ryckmans's reasoning method is as aberrant as that which would claim the bull to be a symbol of death or of the alledged afterlife on the basis of the numerous bucrania found in cemeteries and especially on funerary stelas, as the magnificent Ja 369 found in Ḫeid bin ʿAqîl, the cemetery of Timnaʿ. Then, Jacques



Ryckmans asks himself aloud (( so that the reader might benefit from his own exceptional delirium)) but "sans parvenir à une réponse adéquate" (p. 74) - What a pity! - why the drain of those tables is located on the top of the animal's forehead instead of through its mouth. How is it possible to be so senseless and blind! The libation table is made of solid material, and the upper cavity normally is very shallow; it is thus quite normal to drill the drain at the level of that cavity; these specifications were obviously selected to keep the drilling work at its minimum. Such a truism apparently is ... outside ... Jacques Ryckmans' comprehension. The only example of two superimposed drains (cf. C. Rathjens, Sabaeica, Hamburg, II, 1955, p. 278, photograph no. 564), mentioned by Jacques Ryckmans, has a very deep cavity, viz 4 cm out of the 7 cm total height (cf. C. Rathjens, l.c., p. 155A). Obviously enough, Jacques Ryckmans paid no attention to that very important information, to which he does not even refer. This particularity explains the piercing of a drain at the bottom level of the cavity, beside that at the upper level, which is the normal feature of those tables. Furthermore and obviously enough, a bronze gargoyle, regardless of its form and the animal head represented, always is hollow and never solid; it is thus quite natural to use the animal's mouth as the exit for the water.

2 - Description of the type. - The type of antiquity in Turner 1 is well-known, viz a squarish slab with a hollow, flat cavity on the top, to which is attached a bucranium with its neck, and in the longitudinal axis of this appendix there is a hollow drain dug at the level of the central cavity. It takes a distorted mind to describe, as Jacques Ryckmans does on p. 69,

a - the rim around the central cavity as "un pourtour réservé [sic!] en relief." "Reserved" for whom and for what?;

b - the cavity itself which "aboutit" (!?) to a drain;

c - the neck of the animal as "une corniche [sic] en forme de parallélipipède" (sic!); how does he know that the throat is not wider than the cervical part?;

d - the horns and ears as "traitées de façon abstraite" (sic!), as if that was not the characteristic of South-Arabian art, and which he had already described when mentioning "un bucrane très stylisé." If he really wanted to give a useful detail, he should advantageously have replaced his two adjectives by a single one, viz cubist.

3 - Jacques Ryckmans refers to YM 590 (cf. also p. 70, fn 14 of p. 73, and p. 78) as the first parallel of the antiquity he publishes; and in fn 1 of p. 69, to C. Robin's 1977 thesis. I take advantage of this reference to denounce once more the scandalous dishonesty not only of Jacques Ryckmans, who voluntarily refuses to refer to my Miscellanées..., XIV (1985), pp. 266-271, which deal extensively with YM 590 = RoPH 168 ((including the accurate measurements of the antiquity, "91.8 x 31 cm,

but 38.8 at the bucrania" and other museographic details]] as well as with Robin's thesis, but also of Robin who apparently distributed to those ready to cajole his copies of the photographs illustrating his thesis; my formal request to get that very same photographic documentation was denied, as explained in my Miscellanées ..., I.c., p. 270. Such a behavior is loathsome to the extreme and turns scientific research into a politically personal pet story. Jacques Ryckmans also applauds what has become the trademark of the laudemus invicem gang by restricting (p. 78) his references to three publications, including Robin's thesis, on the meaning of words, such as mslm and mṣrb: my Miscellanées ..., l.c., pp. 266-269, which deal with that problem and suggests new interpretations, are again haughtily eliminated by Jacques Ryckmans.

4 - The second parallel given by Jacques Ryckmans is a small antiquity of Maḥram Bilqîs which "n'atteignait pas 6 cm dans le sens longitudinal" (p. 69), viz Ja 848. Here again, Jacques Ryckmans gives the correct page of my Sabaean Inscriptions ... ,"p. 251," but adds that the length of the object is "5.7 cm" (cf. fn 2 of p. 69). What a fantasy! The information given by my Sabaean Inscriptions..., p. 251B, l.2, is very clear: "plaque: 7.2 x 3.8" cm. How is it possible for Jacques Ryckmans to ... invent a measurement since "5.7" is inexistent in pp. 248-252 of my volume? This votive libation altar (cf. my volume in p. 251A) is still referred to in fn 5 (p. 70) among the antiquities with horns and ears as "proéminences informes." Jacques Ryckmans failed to see that the four protuberances are exactly on the same places as in Turner 1, but instead of being squared, the frontal ones in both antiquities are truncated quadrilateral pyramids, and those on the back of Ja 848 are triangular prisms.

5 - Jacques Ryckmans refers (cf. fn 2 of p. 69) to two other small parallels, viz Maḥram Bilqîs 123 and 129 (cf. W.F. Albright in R.L. Bowen, Jr. et al., Archaeological Discoveries in South Arabia, Baltimore, 1958, pp. 274B and 275A). If Jacques Ryckmans gives the length of the fragmentary MB 129, he fails to give the same information on the first, the length of which is 11 cm. Of course, no reason is given to justify such an exception and all the more so since the length of an antiquity is most important to him as illustrated by the following example.

6 - The last parallel given by Jacques Ryckmans is CIH 692 (cf. fn 2 of p. 69) = AO 1543. Jacques Ryckmans notes that "la mesure de 95 cm [given by CIH] résulte d'une interprétation erronée de celui-ci [= the squeeze], qui moule à la suite les 4 faces d'un petit autel d'environ 22 cm de côté!" It is Jacques Ryckmans who is responsible for a gross miscalculation. The information of "0$^{m}$, 95 lata" of CIH (III, p. 115) is an error of either the copy or the printing because the width of 0.095 m given by the editor H. Derenbourg in RAAO, 6 (1904), p. 44, no. 27 has been repeated



by RÉS 655 (cf. RÉS, II [1907], p. 81) and M. Hartmann in ZA, 21 (1908), p. 15. Furthermore, it is totally out of place to refer, as Jacques Ryckmans does, to a cubic votive block in a study of libation slabs with bucranium spouts. Finally, Jacques Ryckmans' claim of that antiquity being "environ 22 cm de côté" is plain nonsense. The photograph of the "Département des Antiquités Orientales. Musée du Louvre" shows an almost square front with the whole left leg broken off; no scale is accompanying the photograph where the object measures 15.9 (horizontal axis) x 14.7 cm (along the left edge of the right leg). Thus, as the height is 9 cm according to the editor, the width should be about 10 cm.

Three more notes should be added to the preceding discussion of CIH 692.

a - The inscription is complete, slmt/yfʿn Salmat [of the clan] Yafaʿān, with the name of the giver. Had there been a letter at the beginning, its lower left end should still be visible on the stone. CIH's restoration of the noun "[m]slmt" is endorsed as "(m)slmt" by M. Höfner, Beleg-Wörterbuch zum ... CIH, Vienne, 1980, p. 99 (endorsed by W.W. Müller's six pages of Nachträge, Berichtigungen und Bemerkungen zum Beleg-Wörterbuch zum CIH, annexed to the preceding volume, where there is no entry on p. 99 of the book). The reading of "[m]slmt" is also endorsed by J.C. Biella, Dictionary of Old South Arabic. Sabaean Dialect, Chico, 1982, p. 335, as a dubious variant of mslm.

b - The inscription with letter height of about 2 cm occupies about 2/5 of the upper section of the upper panel, and the lower panel is composed of a triptych of animals. On each extremity, there is a walking ibex the body of which is turned toward the exterior of the block with the head facing the central piece, where a standing lion is looking to the left, with a very heavy body and neck as well as flattened paws and especially a horse-like well-rounded haunch. For the contortive position of the two ibexes, cf. also the gem Ja 5282 = RÉS 2690 where the two ibexes are standing on their hind legs (cf. below, pp.164-168) [[ cf. note on p. 116]].

7 - F.P. Albright stated that "many altar tops," of which "the end of the spout was usually decorated with a conventionalized bull's head" measured "from a few centimeters to several meters" (cf. BASOR, No. 128, December 1952, p. 34). Jacques Ryckmans, who reproduces "several meters" in italics to better ridicule F.P. Albright, remarks that "On voit mal ce que vise" the excavator's statement (p. 69). It is rather easy to understand Jacques Ryckmans' blinding knavishness objecting or disbelieving the testimony of a honest archaeologist reporting his findings, an objective reality that he apparently is unable to understand. Those polished slabs, small and large,

were understandably the hottest items during the excavation campaign because practically everyone could use some in his own home; they usually were easily transportable; and the openly antagonistic attitude of Zeid ʿInân and his companion Maḥsen made it impossible for the expedition to protect those antiquities, especially in the evening and at night. Both Albright and myself were there ... and how could Jacques Ryckmans dare to openly and gratuitously ridicule Albright's testimony? what an effrontery! Finally, Jacques Ryckmans claims (p. 69) that Albright's "affirmation [is] reprise par Forte" (cf. M. Forte in AION, 17 [1967], p. 99, fn 6). Here again, Jacques Ryckmans manifestly distorts a printed word. Forte does NOT elicit her own judgment; she merely reports M. Höfner's opinion; here is Forte's text: "M. Höfner, *Forschungen in Südarabien von 1936 bis 1952*: *AfO*, 16 (1953), p. 364 (alcuni di questi esemplari possono arrivare a vari metri di lunghezza);"

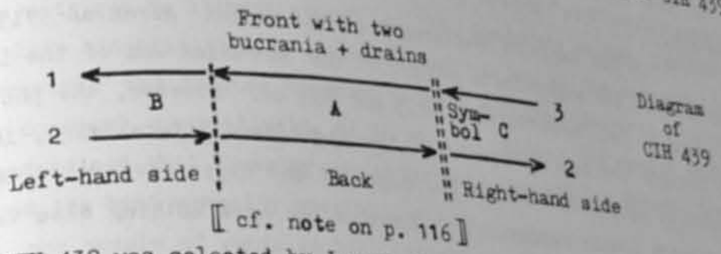
Jacques Ryckmans' deviousness manifestly attributes to Forte the opinion of Höfner. Of course, Jacques Ryckmans would never inform his reader that M. Höfner did endorse F.P. Albrigh's archaeological report (after all, Albright was the excavator, and I myself saw countless of those slabs of all sizes) because he would have to justify his awkwardly disguised rejection. Jacques Ryckmans obviously continues his life-long ploy coined in the famous adage: "Mentez, mentez, il en reste toujours quelque chose." A final remark may be added here. In the title of Forte's paper reported in p. 80 of Jacques Ryckmans' prose, the latter erroneously writes "sudarabici" instead of "sud-arabici," an orthograph which is printed not only in the title (p. 97), but also in 30 other places (e.g., pp. 99 and fn 6, 101 [twice] and fn 13; etc).

C - The engraving of the inscription.

The characteristic of the inscription Turner 1 is that the letters engraved on the two sections of the front side flanking the base of the neck of the bucranium take up the whole height of the available stone and, thus, are 1 1/2 times taller than those chiseled in the two lines of the three other lateral sides of the slab. Incidentally, the right-hand section of the front is erroneously called by Jacques Ryckmans "l'épaule gauche de l'animal représenté" (p. 75, fn 21). There is NO animal represented here; only the head and the neck of a bull are, and contrary to good common sense, Jacques Ryckmans looks at the plate ... from behind!!!, as if, for Jacques Ryckmans, the tail of a bull was the front of the animal. Of course, it is a question of direction and also of taste. Then, after pointing out that the last word divider of 1.2 is next to the word divider following the last tall letter of the incipit as if that was an anomaly disproved by the symmetry of the antiquity, Jacques Ryckmans notes that "on peut noter une irrégularité comparable" in CIH 439 = BM 125067



(cf. my Miscellanées..., II, p. 39B). Nothing of the sort is found in CIH 439 where the two-line boustrophedon text runs only on three sides, lateral and posterior; l.1 begins on the back and continues on the adjacing side where l.2 starts below the end of l.1 and treads its own way through the back side. The last 15 letters which could not be engraved for want of space and the bucranium design are found on the other lateral side; and the second half of them, the very last seven letters, are engraved at the level of l. 1. As far as the text is concerned, it is impossible to justify Jacques Ryckmans' so-called parallel with the present text. The schema of CIH 439 is similar to that of ScS 6 (cf. below, p.115).




[[ cf. note on p. 116 ]]

The case of CIH 439 was selected by Jacques Ryckmans because CIH (II, p. 134) posits that the initial section of the inscription is found "in fronte monumenti" and, consequently, Jacques Ryckmans labels the right-hand side of Turner 1 "le côté gauche" (p. 71, fig. 1); cf. already above, p. 96. The first section of CIH 439 is manifestly on the back of the object. Noteworthy is that CIH - and Jacques Ryckmans follows suite - fails to mention the presence of "two spouts or channels" on the side opposite the back with the beginning of the inscription (cf. S. Birch and A.W. Franks, Inscriptions in the Himyaritic Characters, London, 1863, p. 5, where the mention of front or back is not given) which obviously determine the front. It would be inconceivable to have such a plaque turned in such a way that the liquid would pour out against the wall. Note also that when CIH uses the word "litteras" (= letters of the alphabet) in his introduction, he also includes the word dividers.

D - The measurements.

The first and easiest question to be dealt with is the general accuracy of Jacques Ryckmans' two facsimiles, viz. fig. 2 and 3. The solution is rather easy since, according to Jacques Ryckmans, "seules sont connues les deux dimentions de la pièce...: longueur 37,5 cm; largeur 27,5 cm" (p. 69). Therefore, the relation is 1.364.

"Fig. 2" (p. 74) delineates the horizontal axis of the antiquity" with the following caption: "La table à libations, vue du dessus, l'après [sic!] une esquisse de Turner."

The measurements of the figure are 5.85 x 4.85 cm; thus, the relation is 1.206. "Fig. 3" (p. 76) is a sketch of the inscription.Nothing of his facsimile is on scale, not only his fancy leveling of the four sections of the front that throws off considerably any possible scale, but even - although this is quite simple - the respect of the proportion of the lateral and back sides, viz

the length of each lateral side: 2.6 cm; and
the length of the back side: 2.7 cm.

Thus, the back side is even wider than each lateral side, exactly the opposite of the reality.

On the basis of the length of the object given as 37.5 cm, Jacques Ryckmans deduces the approximate height of the bucranium and of the letters on the lateral sides to be, respectively, 12.5 and 3.2 cm. However, the photograph, which does not include the back extremity of the right-hand lateral side, is very aslant and downward so as to show the hollow depth of the upper cavity; it thus renders all measurements doubly approximate. However, we have nothing else to go on without any hope of a better information.

It should be noted that Jacques Ryckmans does not give the slightest clue or hint as to how he arrived at his measurements. The right-hand lateral side visible on the photograph (p. 71) is 6.7 cm long, to which 0.2 cm should be added, belonging to the extreme right edge of the plaque cut off by the photographer or the printing of the photograph (Jacques Ryckmans does not mention the question and, thus, takes no heed of that material fact); thus, 6.9 cm in length. The width of the lateral side of the bucranium is 2.7 cm. The total length of the plaque is, thus,9.6 cm. As the actual length of the plaque is given as 37.5 cm, the relation between the actual measurements and those on the photograph of fig. 1 (p. 71) is 3.906. Here follow the other measurements:

bucranium : height: front: 3.5 x 3.91 = 13.68 cm;
back: 2.2 x 3.91 = 8.6 cm;
length: 2.7 x 3.91 = 10.55 cm;
plaque: length: 6.7 + 0.2 = 6.9 x 3.91 = 26.97 cm;
width: 2.1 x 3.91 = 8.2 cm;
space above, between and below the letter: 0.1 + 0.15 + 0.1 = 0.35 x 3.91 = 1.36 cm;
height of the letters: small: 2.1 - 0.35 = 1.75 : 2 = 0.87 x 3.91 = 3.4 cm;
tall:section of the front is very aslant; the only clear letter, m, is 1.4 cm hight, and the stone 2.2 cm; thus, the proportion is 0.63; Jacques Ryckmans gives about 7/10.



E - G. Turner's copy of the text.

Jacques Ryckmans deemed it advantageous to himself to retain and hide G. Turner's original copy, thus depriving the reader from the first-hand documentation and forcing him to read his crazy interpretation and, ultimately, making it impossible to check the accuracy of his remarks. Instead, he published his own "légère-ment amendée" version (= "Fig. 3." in p. 76); and his corrections and additions are listed in 17 lines of pp. 75-76. But, these lines, which he imposed upon his reader to satisfy his own ego, cannot be substantiated and raise many questions. His last change is the introduction of a new labeling system "pour rendre compte de l'agencement asymétrique de l'inscription" (p. 76). Obviously enough, Jacques Ryckmans does not explain his criticism of asymmetry and how he, as the engraver, would have achieved symmetry. Had Jacques Ryckmans pointed out that the parts of small and tall letters are composed, respectively, of 70 and 13 units, the reader would have immediately understood that symmetry was materially impossible. Indeed, the carver almost produced the best symmetry possible, viz

first, one couple of words in tall letters on either side of the bucranium itself, viz sdqm/bn/ (6 letters) and /ʿm²ns/tn/ (7 letters); as the father's name has one letter more than his son's, the carver reported one letter followed by a word divider to the left-hand side; what else could he have done? (Jacques Ryckmans does not even ask the question!) and

secondly, he divided the 70 remaining letters into two lines of 35 small letters each.

What the carver could have done to achieve symmetry in his engraving is to carve t of dt/ḥmym on the left-hand side instead of on the back side; then, he would have had 12 letters on each line of the back side and 10 on the left side.

Jacques Ryskmans speaks of "les restes de la barre de séparation avant le mot wqnyhw" of l. 2. Unfortunately, those so-called "restes" are neither described by him nor seen on the photograph because the split in the stone is exactly where the word divider was, viz at equal distance between the two w's. Therefore,Jacques Ryckmans' so-called improved copy erroneously locates that crack almost against the right side of both the 2nd w of wldhw (l.2) and the 2nd m of mlḫmw (l.1). Moreover, Jacques Ryckmans' statement on the word divider is also contradicted by his "[ ]" after wldhw in his transcription (p. 76) since the double brackets are reserved for completely destroyed sign. Finally, he mentions "des irrégularités - dues certainement à la copie, vu le caractère très soigné de la graphie" (p. 75). If so, why does he maintain them in his so-called improved facsimile if it is not for the

The measurements of the figure are 5.85 x 4.85 cm; thus, the relation is 1.206. "Fig. 3" (p. 76) is a sketch of the inscription. Nothing of his facsimile is on scale, not only his fancy leveling of the four sections of the front that throws off considerably any possible scale, but even - although this is quite simple - the respect of the proportion of the lateral and back sides, viz

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tall: section of the front is very aslant; the only clear letter, m, is 1.4 cm hight, and the stone 2.2 cm; thus, the proportion is 0.63; Jacques Ryckmans gives about 7/10.



E - G. Turner's copy of the text.

Jacques Ryckmans deemed it advantageous to himself to retain and hide G. Turner's original copy, thus depriving the reader from the first-hand documentation and forcing him to read his crazy interpretation and, ultimately, making it impossible to check the accuracy of his remarks. Instead, he published his own "légère-ment amendée" version (= "Fig. 3." in p. 76); and his corrections and additions are listed in 17 lines of pp. 75-76. But, these lines, which he imposed upon his reader to satisfy his own ego, cannot be substantiated and raise many questions. His last change is the introduction of a new labeling system "pour rendre compte de l'agencement asymétrique de l'inscription" (p. 76). Obviously enough, Jacques Ryckmans does not explain his criticism of asymmetry and how he, as the engraver, would have achieved symmetry. Had Jacques Ryckmans pointed out that the parts of small and tall letters are composed, respectively, of 70 and 13 units, the reader would have immediately understood that symmetry was materially imposible. Indeed, the carver almost produced the best symmetry possible, viz

first, one couple of words in tall letters on either side of the bucranium itself, viz ṯdqm/bn/ (6 letters) and /ʿmʾns/bn/ (7 letters); as the father's name has one letter more than his son's, the carver reported one letter followed by a word divider to the left-hand side; what else could he have done? (Jacques Ryckmans does not even ask the question!) and

secondly, he divided the 70 remaining letters into two lines of 35 small letters each.

What the carver could have done to achieve symmetry in his engraving is to carve t of dt/ḥmym on the left-hand side instead of on the back side; then, he would have had 12 letters on each line of the back side and 10 on the left side.

Jacques Ryskmans speaks of "les restes de la barre de séparation avant le mot wqnyhw" of l. 2. Unfortunately, those so-called "restes" are neither described by him nor seen on the photograph because the split in the stone is exactly where the word divider was, viz at equal distance between the two w's. Therefore, Jacques Ryckmans' so-called improved copy erroneously locates that crack almost against the right side of both the 2nd w of wldhw (l.2) and the 2nd m of mlḥmw (l.1). Moreover, Jacques Ryckmans' statement on the word divider is also contradicted by his "[ ]" after wldhw in his transcription (p. 76) since the double brackets are reserved for completely destroyed sign. Finally, he mentions "des irrégularités - dues certainement à la copie, vu le caractère très soigné de la graphie" (p. 75). If so, why does he maintain them in his so-called improved facsimile if it is not for the

morbid enjoyment e of having something to criticize? Finally, Jacques Ryckmans constantly betrays "le caractère très soigné de la graphie" by repeatedly shaping most carelessly his letters which are not recognizable when compared with those of the original; e.g. all his w's and the upper part of ẓ, y, ḳ are ugliy elliptical instead of being circular. More details follow in the next section.

F - The palaeography.

Jacques Ryckmans' palaeographical description of the text (p. 76) calls for the following remarks.

1 - He claims that the transversal axis of the cut is "triangulaire très nette." It actually is impossible to detect on the photograph whether that axis is ⌄, V or U.

2 - He speaks of the "jeu des ombres dans l'incision" of the letters which gives "par endroit" the impression of the well-known serif at the extremities of vertical staffs. But, he does not give a single example of his vision, and not a single case is visible on the photograph. That impression, if it does exist, would be nothing but a visual illusion caused by the shade in the cut regardless of the shape of the cut itself.

3 - He claims that the central bar of n "est horizontale, ou forme un angle légèrement obtus." As there are only two n's on the photograph, the list of his examples would have been very easy to make. Yet, no reference is given, and the bar is perfectly horizontal in both cases. Besides, "un angle légèrement obtus" would mean ⌙ (unknown in the monumental script) instead of ⌙ (acute angle) which is normal. There is, thus, the confusion between "acute" and "obtuse" as there has been, before, confusion between front and back (cf. above, p. 96) and between "au-dessus" and "au-dessous" (cf. above, p. 90).

4 - He claims that "la pointe des deux triangles du m, bien séparés, est à angle aigu, et ces deux triangles forment entre eux un angle aigu." In this quotation, the word "la pointe" should be replaced by "le sommet;" and "bien séparés" is unintelligible since the triangles of m normally are contiguous to each other. Here again, the check is very easy because there are only the m's of mlḫḥmw (l. 1) to be seen on the photograph. The m of ṣdqm, that Jacques Ryckmans may (?) describe in his statement, cannot safely be used since that section of the front is very aslant on the photograph. In the two other m's, the two triangles are equilateral, the width of which is half of the letter height and equals the distance between the two vertices. Jacques Ryckmans distorts them by making them too slender. Cf. also below, p. 102, YM 6 and CIH 439.



5 - "Le trait oblique de l est très long," remarks Jacques Ryckmans. Such a form is normal in that palaeographical type and, here again, his so-called improved copy of the text completely distorts the two l's visible on the photograph, the first by running the outside extremity of the aslant appendix below the right section of the cup of ḫ, and the second by making that appendix so short that it could not be shorter without vanishing.

6 - Jacques Ryckmans states that "les oeillets des lettres sont relativement petits." Here again, he fails to explain what "relativement petits" means in his jargon. In fact, the diameter of those circles is somewhat less than 1/3 of the letter height, and Jacques Ryckmans represents the circle of w, ẓ, ḳ and ṯ much too small and obliquely elliptical, although they obviously are circular, as shown on the photograph of p. 71.

7 - Jacques Ryckmans compares the letter type of this inscription with that of "RÉS 4635, dans la période A 3 de Pirenne." But, none of the four samples of Pirenne's A3 in her "tableau 2" fits the palaeography of RÉS 4635. For my analysis of Pirenne's A3, 3rd and 4th lines, cf. my La paléographie sud-arabe de J. Pirenne, Washington, 1957, pp. 76-81, to which Jacques Ryckmans is not honest enough to refer. Furthermore, Jacques Ryckmans' superficiality in the present case is such that it cannot but be considered as another case of dishonesty. The present text gives yet another proof of the fundamental error of Pirenne's system, viz the rejection of "la contemporanéité de types graphiques dissemblables" (cf. l.c., p.35). Indeed, the tall letters belong to a type so different from that of the short letters - a difference that Jacques Ryckmans apparently is incapable of seeing or refuses to see - that they could not possibly coexist in Pirenne's imaginative scaffolding, viz proportion of { the short letters: 1/2; but of the tall letters: between 1/4 and 1/5.

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9 - Jacques Ryckmans refers to both YM 6 and CIH 439, "dont la graphie exhibe un m aux triangles à angle obtus: forme qui apparaît seulement plus tard" (p. 76). I denounce most strongly not only this description of Jacques Ryckmans as grossly erroneous because there is not a single "angle obtus" in YM 6 and there are more sharp and square angles in CIH 439 than obtuse ones, but also Jacques Ryckmans' loathsome simplism of reducing the characteristic of a text to a single letter only. And my denounciation is all the more forceful that he only has at his disposal to study Turner 1 a single partial, aslant photograph. I have at my disposal the pencil rubbings of YM 6 = Ga 65, and the enlargements of BM 125067 = CIH 439. The three texts cannot be compared with each other without reliable, detailed photographic documentation on Turner 1.

The general characteristic of CIH 439 is that the proportion of the letters oscillates around 1/3; for some remarks on the layout of the text, cf. above, p. 97. The letter type of YM 6 = Ga 65 (cf. G. Garbini, in AION, 33 [1973], p. 31) shows the following characteristics: some m's are slightly open (e.g., in ʾlmqh and mslm); the upper part of h and ḫ shows two square angles (cf. nḫlhw); the letter relation of 1/3 is found in b and d (cf. bmrʾ and dmdyn), and that at equal distance between 1/2 (as in y of ydʿʾl) and 1/3 is seen in some ʾ, h, ḫ, y, l, n and s. - Museographic details on YM 6 missing in Garbini's publication. Plaque: 85.1 x 36.4 cm, and 44.6 on the bucrania; width of the rim around the upper cavity: 3.8 cm; depth of that cavity: 0.5 cm; each drain: 12 x 3.5 (at the cavity) and 1.5 (at the bucranium) cm. Letter height: 5 cm; spacing: 1 cm; distance to the edges: 1.1 (to) and 0.9 (right) cm. Four corrections must be reported to Garbini's transcription of the text (cf. l.c., p. 31): add a word divider at the end of both ll. 1 and 2; suppress the word divider after dmdyn, and correct "ṣmnt" (pp. 31, twice and 32, twice) to smnt, as the feminine name of RÉS 3957/1 and 9.

Jacques Ryckmans' facsimile is a travesty of the original and is rejected out of hand. What a pity that his insatiable arrogance prevents the reader from studying Turner's original facsimile which would have been most welcome! Although Jacques Ryckmans' facsimile of the text would have greatly helped the reading of this paper, I shrank from reproducing his so-called facsimile, defaced, as it is, by so many unexplainable fantasies.

G - The text.

| | right-hand side | back | left-hand side ← | front ← | |
|---|---|---|---|---|---|
| 1 | ww/wmhḥlm/tybẓd | /ʿmsw/rttʿ/ynqh/ | ʿt.y/nb/mwyḥ | /nb/snʾmʿ/ bucranium /nb/mqdṣ | 1 |
| 2 | ldhw/wqnyhw/bʿt | tr/wb/ʾlmqh/wb/dt | ḥmym/wb/smʿ/ | | |
| | → | | | | |



1 Ṣadiqum, son of bucranium ʿAmmʾanas, son of Ḥaywum, of [the family] Ya.taʿ, has dedicated to ʿAttar and Samîʿ, Him of Ẓabyat, [his wife] Mâliḥhumû and
2 his children and his slaves. By ʿAttar and by ʾIlumquh and by Dât-Ḥimyâm and by Sâmiʿ.

1 - Cheepishly following in the footsteps of C. Robin, Jacques Ryckmans eliminates from his transcription of the text all word dividers, save the last one which has as much value as any other. But, this procedure leads him twice into error. a - Just looking at his prose no reader would ever suspect that between bn and ʿmʾns there are two word dividers that are not accounted for in his transcription; and

b - His transcription indicates that, for him erroneously, there is a word divider between dt and ḥmym since they are separated from each other by a blank, contrary to his transcription of ʿmʾns, bn, y[]tʿ and bʿttr.

2 - The author's identity card involves four names, the last three being introduced each by bn. Jacques Ryckmans' commentary (p. 77) first refers to "Robin, Hautes Terres, I, p. 17; Problème, p. 47." These two references are mere flattering infillings: the preceding type of identity card is not even mentioned there. Secondly, Jacques Ryckmans refers to three texts which, according to him, contain the same type of identity card, viz "RES 4808+4811, Ja 2848 t, et 2848 aL" (cf. fn 23 of p. 77).

The first reference should be corrected to RÉS 4811+4808, another case (cf. already above, p. 96) where Jacques Ryckmans considers the tail to be the head. And the third should be also corrected to Ja 2848 aL/2-3.

Furthermore, the three references also are erroneous because the identity card of RÉS 4811+4808 is x/BN/y/BN/z, viz only two bn's and no family/clan/tribe affiliation, and

Ja 2848 t and aL/2-3 is x/BN/y/BN/z/d + tribe name; thus d instead of the third bn.

The summum of impudence is that Jacques Ryckmans gives the correct pages where each of those three texts is published; therefore, in all the five preceding instances, although giving the correct pages, Jacques Ryckmans proves himself unable to acknowledge what he sees; instead he reads what he chooses to read.

3 - The offering of this libation slab was to celebrate the author's dedication to his two most favored deities, of mlḥhmw/wwldhw/wqnyhw which Jacques Ryckmans translates as "Mlḥhmw ainsi que sa (propre) descendance et son avoir" with mlḥhmw as the "nom d'homme de la personne offerte" (as if it were the only one; cf. also p. 78), also with "sa (propre) descendance" by opposition - I presume - to

mlḥhmw's progeny, the whole text being "une formule plutôt symbolique" (quotations from p. 77). This is an example of Jacques Ryckmans at his best, viz voluntarily vague and without any explanation in order to be able later on to easily explain his lingo in an expedient manner and, in so doing, to extricate himself from any given objection. Indeed, what could possibly be the real meaning of "formule plutôt symbolique"? The only certainty is that, for Jacques Ryckmans, the formula does not express the real fact but is, I suppose, a mere symbol, a literary echo of an unknwon - if existing at all - reality, a mere wishing thinking; in fact, Jacques Ryckmans expresses his atheistic understanding of a religious ceremony. Of course, Jacques Ryckmans does not justify his unacceptable opinion. The meaning of the verb hqny has dedicated implies the creation of special bounds between a human being and certain deity/deities. The verb cannot refer to human sacrifice, as Jacques Ryckmans and consorts might sometimes have in mind, because hqny and dbḥ are not synonyms. The nature of those personal bonds, their implication and their ceremony, however, remain unknown to us.

4 - In spite of the relentless verbosity shown in his paper, Jacques Ryckmans does not make a single remark on the nature of the two gods receiving the dedicace: the stellar ʿAttar and the lunar Samîʿ (cf. my paper in Le Muséon, 60 [1947], pp. 70-71, 85 and 112). The present text with ʿAttar's name preceding that of the lunar god testifies to the personal devotion of the author and, therefore, cannot imply the priority of the first god over the second in the divine triad.

5 - Jacques Ryckmans states that "la portée exacte de pareilles dédicaces reste imprécise" (p. 70). Such an affirmation openly contradicts his former statement on "une formule plutôt symbolique" because a mere symbolic formula cannot have any real bearing. Here again, Jacques Ryckmans shows how superficial all his garrulity really is. The answer to his problem rests upon a simple remark: an individual simply could not dedicate to the deity someone upon whom he would not have full authority. Yet, Jacques Ryckmans does not even attempt to figure out what kind of dependency mlḥhmw might have toward sdqm because to simply mention that that person is a man, as he does, shows fanatic chauvinism and is an empty babbling. Furthermore, as wld precedes qny, the first apparently seems to be more precious to the author than the second. Similarly, mlḥhmw must be somebody closer to sdqm than even his wld, since the word introduces the series. It may thus be safely stated that the name of mlḥhmw is that of sdqm's wife.



6 - Jacques Ryckmans' translation of wld as "descendance" is erroneous. If an individual has full authority on his children (boys and girls) + wld, he has none on his grandchildren, who depend on their own father.

7 - Jacques Ryckmans' translation of qny as "avoir" (cf. also Sabaic Dictionary.., p. 106, first meaning) is incorrect. In contexts, such as this, qny means slave(s) (cf. my Sabaean Inscriptions..., p. 447B) because it is incomprehensible that the dedication of inanimate things would be on an equal footing as that of persons.

[[cf. note on p. 116]]

Therefore, the three words illustrate the preceding trilogy: "[his wife] Mâliḥhumû and his children and his slaves (cf. already, p. 103). For mlḥhmw, cf. Sabaean ymlḥʾl of Ja 2942a.

8 - The formula "par laquelle le dédicant consacrait (en outre, éventuellement, sa propre personne) sa descendance et son avoir, est également attestée, avec de légères variantes, dans ... Gl. 1519, 1523, 1524 et Gl. A 768" (p. 77). This is an outstanding example of how much Jacques Ryckmans distorts the meaning of very simple words.

First of all, Gl 1522 should have been added to the four already listed. But, the very fragmentary Gl 1524 has no value in itself because it is restored on the basis of the identical Gl 1523; and that restoration should include wl[dhmy/wqn(7)ytmy] on the basis of Gl 1523/6-7, instead of "wl[dhw/wqnyhw]," as in H.Höfner und J.M. Solá Solé, Inschriften aus dem Gebiet zwischen Marib and dem Golf, Vienne, 1961, p. 25. Furthermore, Gl A 768 also is very fragmentary. In Gl 1519 and 1922, hqny introduces respectively wldhw and hrm, which is the name of the author's wife. Only Gl 1523 is a valid parallel, where ʿmdrʾ is the name of the author's eldest son.

H - Jacques Ryckmans' "Addendum" (p. 78, 25 lines).

Jacques Ryckmans' addition of a lengthy appendix to his paper undeniably intended to prove him to be quite up-to-date. Unfortunately, such a purpose did not prevent him from omitting an exceptional example of libation slab, AO 20.179 (cf. Y. Calvet et C. Robin, Au royaume de Saba: archéologie du Yémen, Paris, 1980, pp. 13B-14A and photograph in fig. 10 of p. 10), where the bucranium with its horns and ears (as in NAM 1308) is at the end of an exceptionally long neck attached to a large plaque built with two recesses on each lateral side in the form of a large, upside-down tau cross.

Another beautiful example of a libation slab with two recesses as above and ornated with quite a unique design of bucranium was also eliminated by Jacques Ryckmans; cf. the two outstanding photographs in A.R. al-Ansary, Qaryat al-Faw, A Portrait of Pre-Islamic Civilisation in Saudi Arabia, Riyadh, 1981, pp. 70-71, nos.

1 and 2 (this no. 2 is further reproduced in pp. 59 = 78).

Jacques Ryckmans refers to p. 54 of W. Daum ed., Yemen, Innsbruck, 1988, which represents a mere votive slab, erroneously called a "table à libations" by Jacques Ryckmans although he, himself, admits that there is no drain visible!!! In fact, there is no drain at all. Furthermore, Jacques Ryckmans claims that the photograph does not exist in the German edition of the preceding volume, viz W. Daum ed., Jemen, Innsbruck, 1987. This information is erroneous: the very same photograph is on p. 55. Note that, contrary to Jacques Ryckmans' p. 80, the title of the German edition is "Jemen" while "Jemen - 3000 Jahre Kunst und Kultur des glüchliken Arabien" is the title of the exposition at which occasion the book was released, as clearly spelled out on pp. 3 and 7 of the book.

The space between the broken horns in the forehead of the bucranium is ornated with at least ten vertical groves delimiting nine folds representing the frontal forelock. It also takes an unbalanced mind to suggest, as Jacques Ryckmans does, that those folds "sont probablement à l'origine des denticules qui apparaissent plus tard." It is well-known that there is no dentils on the forehead.

In his reference to ScS 9 of Archäologische Berichte aus dem Yemen, IV (1987) 1988, Jacques Ryckmans erroneously mentions "pl. 33 b, c" instead of 35 a ,b, a reference that he, himself, mentions two lines further down. Jacques Ryckmans, then, states that ScS 9 "paraît fort incomplète" as the section of the text still preserved "ne représentait dès lors qu'une bonne moitié de l'original." At least, four letters are missing in the front, thus "/hqny." As the verb normally requires a complement and the right-hand side has 9 letters, the left-hand side could have wdm/ dmsm$^{c}$m, which is exactly 9 letters. The two preceding restorations are fairly certain, and the second restoration is parallel to ScS 6 (cf. Archäologische..., l.c., p. 185) ... if the table only has one bucranium. Whether or not something was engraved on the left-hand section of the back remains unknown. If yes, no more than 4 letters are possible. Therefore, 13 and 17 letters are missing, compared with the 19 of the existing text. However, there still remains the possibility of the table having two bucrania, already suggested by Jacques Ryckmans, in which case the number of missing letters cannot be evaluated since the distance between the two heads would be unknown. Finally, according to Jacques Ryckmans, a photograph "en perspective plongeante de la table" was sent to him by W.W. Müller, "d'où il paraît ressortir que le liquide était évacué par un orifice partant du bord latéral de la table et débouchait dans la gueule de l'animal." This confuse, unprecise statement is typical of Jacques Ryckmans' lingo: what is meant by "le bord latéral de la pla-



que" and where is the exact location of the opening? No such drain is mentioned by J. Schmidt's description of the plaque in Archäologische..., l.c., p. 184. How could Jacques Ryckmans see on a photograph taken aslant from above a drain existing in the mouth of the bucranium, that cannot be seen in any photograph except if taken from below? Here again, note the loathsome habit of those laudemus invicem gang members flattering and favoring each other at the expense of true research and all the more so that the famous photograph is not even published in Archäologische ..., V (1991), where, I F Müller actually had really wanted to publish an additional photograph - but objectivity has never been the actuating goal of his life - he could and would have done it easily. Nonetheless, Jacques Ryckmans concludes saying that symmetry would then demand the existence of two drains, "comme les exemples," so goes his claim, of CIH 439, YM 6 and 590. But, these three examples do not prove his point: each of them has two (not one) heads on the same side of the antiquity. Besides, how could there be two drains since there is only one mouth? The case of ScS 9 is the same as that of Hôr Hôrî (cf. above, p. 92). The only reference Jacques Ryckmans should have given is Hambourg, photograph 564 (cf. above, p. 93), where the two drains are clearly seen on the photograph, but one of them is, as usual, at the top of the bucranium.

I - Notes on the photographs of W. Daum ed., Yemen..., pp. 50-54.

The photograph of p. 54 illustrates W.W. Müller's contribution to an "Outline of the History of Ancient South Arabia" (pp. 49-54), which incorporates H. von Wissmann's opinion on the early phase of Saba', which I have always maintained in the past even against J. Pirenne's mini-chronology.... and I was the only one! Of course, Müller knows that. But, evidently, he would not acknowledge the truth unless it is expressed by someone who flatters him or whom he may need to promote his future. As stated above, the truth has never been the intent of his research. Müller's paper is also illustrated by two other photographs which contain a rather illuminating lesson, viz

p. 50A: photograph of CIH 362: both the caption and the transliteration of the text is signed "W.W.M.;"

p. 51B: photograph of CIH 445: only the transliteration and translation of the text are signed "W.W.M.;"

p. 54: photograph of the front of ScS 9 referred to by Jacques Ryckmans in his "Addendum;" no "W.W.M." signature is found there.

No reason is given why Müller adopts a position different in the three cases.

CIH 362. - Could it be possible that for W.W. Müller, who always gives the impression of being "the long-awaited messiah in interpreting SA texts" (cf. my

Miscellanées ..., XIV [1985], pp. 134-137) this short inscription remains "obscure" as he had already admitted in Sabaic Dictionary ..., p. 32, where bry is at least considered as a verbal form. But, he apparently failed to convince his three other partners of the "laudemus invicem" gang of his 1962 etymology (cf. Die Wurzeln mediae und tertiae Y/W im Altsüdarabischen. Eine etymologische und lexikographische Studie, Tübingen, 1962, p. 29). But, he may find some consolation in the fact that J.C. Biella, Dictionary..., p. 57, endorses his theory. However, Müller had already abandoned his opinion before 1980 because p. 2 of Nachträge .... (cf. above, p.99) failed to mention bry. I maintain my etymology presented in Cahiers de Byrsa, 8 (1958-1959), pp. 173-174.

CIH 445. - The caption of W. Daum's Yemen... presents the antiquity as "Sabaean stele (altar?)." Had the caption mentioned that the width of the antiquity is 9 cm (cf. already J. and H. Derenbourg in RAAO, 1 [1885], p. 57, repeated in RÉS 635 of RÉS, II [1907], p. 67), the writer of the caption might hopefully have seen how ridiculous it is to consider a 9 cm (= 3 in 9/14) wide rectangle as an altar top. — The caption statement on a "lancer on a camel (caravan guard)" is imaginative, but groundless. CIH (II, p. 144) explicitely summarizes J. Mordtmann's analysis of the stela in ZDMG, 35 (1881), p. 433; also referred to by RÉS, II, p. 67, no. 635, which speaks of a mounted horseman preceded by his camel, a scene which must have been repeated many times every day, viz an Arab on horseback with his favorite weapon, his everyday companion and following his camel. — The caption also gives the two measurements "35 x 23.7 cm;" thus, the proportion is 1.47. These measurements are erroneous, no matter were they come from. Ever since J. and H. Derenbourg's paper in 1885 and RÉS 635 (1907), the measurements are "55 x 29 cm. The measurements of CIH's photograph, viz 29.5 x 15.5, and of the museum photograph, viz 22.1 x 11.3 cm, give both the proportion of 1.9, and this is irreconciliable with 1.47 of the caption. Yemen...'s measurements are so aberrant that W. Daum must have copied them from a misplaced file. Furthermore, CIH failed to mention that the letter height is 2.5 cm, as already given by J. and H. Derenbourg and repeated by RÉS. — Müller translates wlqmʿn/ʿttr/srqn as "And Attar Šariqan may smite(or:deter)." Five errors must be corrected in this extremely short pericope.

a - The spelling "Attar" is erroneous because "A" normally represents "ʾA" in the transliteration of arabicists, but never "ʿA;"

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d - The verb qmʿ does not mean either "smite" or "deter," which, as "overthrow" of Sabaic Dictionary ..., p. 105, are imaginative or lyrical adaptations of qmʿ = "subjugate" (cf. my Sabaean Inscriptions..., p. 447B, and J.C. Biella, Dictionary..., p. 458).

Müller translates hrš as "destroy" (cf. also Sabaic Dictionary..., p. 82) which, here again, is an imaginative adaptation of "obliterate" (cf. also J.C. Biella, Dictionary..., p. 212). Finally, his translation of the imperfect yuršn as "who wants to destroy" needlessly and wrongly introduces an absent vert into the South-Arabian formula, "to want;" the verbal form is adequately rendered as "who would obliterate." South-Arabian deals with facts, not with intentions or wishes.

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the letter m is not close in ScS 6, 9 and 12, another datum which contradicts Pirenne's straitjacket scaffolding.

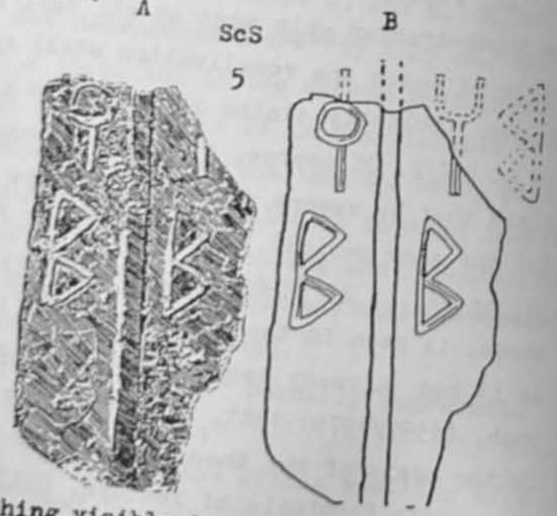
J - Further notes on W.W. Müller's handling of ScS 6, 7 and 12.

1 - Introduction: ScS 5.

The study of ScS 5 is inserted here because it exemplified J.Schmidt - W.W. Müller's collaboration, which treats the fragment ScS 5 almost as a nullity by refusing to give that precious fragment the attention it deserves and by limiting their effort to a mere careless presentation in Archäologische..., I (1982). As a matter of fact, the caption of its facsimile does not even mention its siglum; the antiquity is simply presented as an anonymous "Inschriftfragment." The siglum is not printed in exergue as the other numbers 1, 2, 3 and 4 in Müller's text; it is lost between parentheses inside Müller's last paragraph of p. 106.

a - The fragment ScS 5 is not illustrated by a photograph, but only by a fancy facsimile in l.c., fig. 29 of p. 96.

If the facsimile is the work of a professional draughtsman as can be seen in the setting of hatchings to match the various spots of the surface, the irregularities of the two m's and the shapelessness of the ellipsoid of 1.1 instead of a circle prove it to be an inaccurate facsimile even though the whole design is rather pleasant to look at. Furthermore, it is also incomplete as the bottom edge of the piece is not even drawn. Finally, the facsimile is not even accompanied by a scale; therefore, the reader cannot be certain of being presented with a design on scale. And since the author was not an epigraphist, the reader cannot be certain that nothing above the ellipsoid of 1. 1 is preserved.




The facsimile of Archäologische...'s fig. 29 is reproduced on the right in A, while B is my tracing of A with my restoration of the 3 upper letters.

b - Schmidt informs the reader (p. 96) that it is a piece of limestone, but gives no measurements at all. Since the facsimile has no scale, nobody can even guess the approximate size of the letters. Schmidt also states that the transversal axis of the grooves is U. However, the hatching visible in the facsimile points to V, instead of to U. But, it should be noted that the hatchings of the facsimile are



partial and inconsistant, another indication that the attention of the draughtsman was neither sustained nor precise. The lines inside the letter forms of my tracing demarcate shade and light. At any rate, those hatchings could only indicate the shade inside the cut instead of the shape of the cut. Nobody knows what the author had in mind when he/she was drawing the hatchings.

c - W.W. Müller. - Being the epigrapher, Müller had the duty to see to it that the fragment was properly published with a scaled photograph. Of course, his inborn flattering instinct dictated him to remain silent less he might upset J. Schmidt. On the other hand, his amateurish attitude toward the few letters of the fragment indicates that he was not willing to waste his time on a broken piece with 3 and 1/2 letters according to his understanding.

Müller's examen, which cannot be called a study, of ScS 5 (p. 106) contains a little more than 4 (four) lines, mostly devoted to the self-indulgent remark that nothing can be gotten from such a hopeless scrap. His initial reflection, however, contains a manifest error. Müller, indeed, speaks of a "Y mit einem grossen, die halbe Buchstabenhöhe einnehmenden Kreis." Although the reality is staring him in the face, Müller still did not see it, viz it is most obvious that on the original facsimile the height of m is 2 cm, but

that of his "Y" is 1.5 cm only provided that the letter height is the same. This means that the upper quarter of the letter is missing, and the original letter is q. What a pity that it did not please J. Schmidt to publish a scaled photograph! But, the main responsability lies with W.W. Müller who, as the epigrapher, had to see to it that the photograph was released and published. Unfortunately he failed. He also failed to give the measurements of both the stone and the inscription. How could he manifest more clearly his disdain of the fragment?

With the sequence of ⌜q⌝mm assured, the name that comes first to mind in the present collection is mhqmm. Thus, I restore the fragment ScS 5 to read as follows: ← ⌜qh⌝[m/nb/....] 1 ...., son of Mu]h)qi-
→ mm[/hqny/wdm] 2 mum, [has dedicated (this) to Waddum.]

The object of the dedication was the building to which the inscribed stone belonged; for its absence, cf. also below, ScS 12 and Ja 3237. - mhqmm is also found in ScS 12 as a masculine name. Beside the preceding parallel, the name that would come first to mind is hqmm, the name of a member of the famous tribe Halil, who wrote one line in the onomastics lists, Ja 2848 ai (cf. my Carnegie Museum 1974-1975 Yemen Expedition, Pittsburgh, 1976, pp. 51, 208A, and tracings in pl. 12 R).

2 - ScS 6 (cf. Archäologische...., IV [1987] 1988 , pp. 185-186 and pl.

33 b and c).

For J. Schmidt's description of the incense burner, but without the measurements of the text, cf. p. 183. There is no cross-reference in Müller's text. Müller spends no less than eleven lines (p. 185) in an attempt to prove that

"Da nur zwei Seiten des Räucheraltars beschriftet sind, wird die Inschrift nur verständlich, wenn man sie in der Mitte der ersten Zeile auf der ersten Seite beginnen läßt."

The statement introduced by the conjunction "Da" does not and cannot in any way justify the main proposition, but it shows how deviously Müller builds up his arguments, and it is on the basis of such an erroneous argumentation that he suggests his crazy understanding of the text as follows:

"

1. (A) *smʿm/qwmm* (B) *yhqm/bn/qf* ←
2. (B) *qfm/dbwr/h* (A) *qny/wdm/dm* →
1. Qawwāmum Yuhaqīm, der Sohn des Qaf-
2. qafum, von (der Sippe) Baur hat gewidmet dem Waddum Dū-Ma=
1. smaʿīm ."

Without informing the reader, Müller fraudulently inverted the two sections of l.1 for the sole apparent purpose of having qwmm in the front. This is an imposture invented to lure the reader into approving his concoction! Here follows the translitteration of the text with the stippling indicating Müller's dissection of the text and also showing that he does not specify to which of his lines the word divider of l.1 of the right-hand side actually belongs.

| Front | Right-hand side | | |
|---|---|---|---|
| ←..... | .l.1 | ? | ←...l.2 B |
| fq/nb/mqhy | mmwq | / | mᶜms |
| qfm/dbwr/h | qny/wdm/dm | | |
| l.2 A.....→ | | | |

Müller's treatment of the text shows two irregularities, viz

a - his text begins in the middle of a line, not at the beginning;

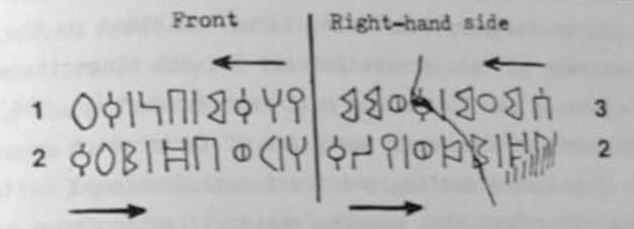
b - the author's name is divided into two sections in such a way that the onlooker would first see the second section of the name.

Müller' error originated from the fact that he did not know what to do with qwmm on the left of "mᶜms," viz he failed to recognize that the text is a dedication of a human being to the divinity. Therefore, the left half of his l.1, qwmm, in fact belongs to l.3,which is engraved at the level of l.1. The schema of the text is similar to that of CIH 439 (cf. above, p. 97); therefore, qwmm, instead of being the male dedicator to the deity, as Müller has it,is in fact the dedicated woman,



as are ʾlkbr of ScS 2/2, [.]sshmw of ScS 4/2 (instead of erroneous "[r]ʾshmw" of Müller),or rʾbm of ScS 7 ; cf. below, remarks on ScS 12.

The following graphic of ScS 6 clearly shows Müller's obvious errors:




| | | | |
|---|---|---|---|
| 1 ← | fq/nb/mqhy | mmwq/mᶜms | ← 3 |
| 2 → | qfm/dbwr/h | qny/wdm/dmʿm | → 2 |

1 Yahqim, son of Qaf- 3 smaʿum, [his wife] Qawmum.
2 qafum, he of (the family) Bawâr, has dedicated to Waddum, Him of Mu-

yhqm, cf., e.g., my Sabaean Inscriptions..., p. 408B. - qfqfm, cf. Arabic qafqef "se blottir, être assis sur le derrière" (cf. C. Denizeau, Dictionnaire des parlers arabes de Syrie, Liban et Palestine, Paris, 1960, p. 428), a parallel not given by Müller. - bwr, a personal name in Sabaean Ja 2940 a; a parallel not given by Müller. - qwmm, cf., e.g., Ja 2918 a; a parallel not given by Müller. Here again, Müller gives another example of his blind pan-classical Arabic fanaticism when he claims that his vocalization of qwmm as "Qawwâm" is justified by al-Hamdânî's al-ʾIklîl, vol. 10, edited by Muḥibb ad-Dîn al-Ḫatîb, Cairo, 1368/1948, p. 247, l. 16 (also in p. 248), where the reader finds "al-qawwâm" (cf. also O. Löfgren, al-Hamdānī. Südarabisches Muštabih, Uppsala, 1953, p. 42, no. 831), which may correspond to South-Arabian qwmn/qwwmn, not to qwmm; cf. Arabic qawâm "iustum, rectum; statura" (cf. G.W. Freytag, Lexicon arabico-latinum, Halle , III (1835), p. 517B). Qawmum is the name of Yahqim's wife; cf. above, Turner 1, Gl 1522 and 1523 for a similar formula. For wdm/dmsmʿm, cf. already Ja 3223/2-3 (to be published).

3 - ScS 7 (cf. Archäologische..., l.c., pp. 186-187 and pl. 35 c-e).

1 - As in ScS 6. for J. Schmidt's description of the votive plaque without the measurements of the text, cf.p.184; there is no cross-reference in Müller's text.

2 - Müller also fails to state that the text begins on the right-hand lateral side and proceeds leftward to the front and the left-hand side.

3 - Müller's edition is particularly narrow-minded and bias, as usual, with another example of his pan-classical Arabic fanaticism and a loathsome case of voluntary distortion of Nöldeke's writing.

a - No remark at all on the very old letter forms exhibited in the text. Müller is interested exclusively in "die grosse Schrift der roh eingeritzten Buchstaben steht in krassem Gegensatz zur Platte aus poliertem Marmor" (p. 186). What actually is "krass" is Müller's confusion of two pairs of facts which have nothing in common, viz the beauty of polished surface and the non-professional letter forms on the one hand, and on the other hand the non-professional letter forms and the style. These three facts cannot be compared with each other. The style of the letter forms which is characterized by the very wide ḥ with a very short stem belongs to the first palaeographical period. This text is a precious example showing the difference between the graffiti lettering (cf. also ScS 12) and the monumental form (cf., e.g., ScS 9) belonging to the same period.

b - Müller claims that the 1st element of the author's theophoric name composed of three letters is "nicht mehr erhalten", viz "...ʾl ...ʾil" (p. 186). Yet, the left section of the third letter m is still clearly visible on the stone; thus, [..][mʾl. Müller also errs when he reads the divine name "wd," an orthograph that does not exist in this series of texts; all of which have wdm; cf. ScS 1/2, 2/1-2, 3/2, 6/2, 10/3-4 and 12; the last m is still clearly on the stone; thus, wdm instead of Müller's erroneous "wd."

c - It is on the question of the name rʾb that Müller really goes overboard.

(1) Here again, South-Arabian rʾbm should be spelled "Riʾāb" because it "erfolgt nach" the Arabic pronounciation. What an empty rubbish repeated all over again!!! And to make it even worst, Müller suggests "Riʾābum."

(2) He claims that the Arabic name "Riʾāb, dessen Bedeutung nach Th. Nöldeke 'Schliessung eines Risses, Herstellung' ist"(p. 186). This is untrue: all Nöldeke speaks of is "رُؤْبَة = palm. rbt" (cf. Beiträge zur semitischen Sprachwissenschaft, Strassburg, 1904, p. 100, l. 22); he does not speak of rʾb at all, and neither Arabic riʾâb nor Palmyrene rbt is South-Arabian rʾbm. There can hardly be any doubt that Th. Nöldeke's explanation of ZDMG, 24 (1870), p. 89, which was endorsed by M. Lidzbarski in Handbuch der nordsemitischen Epigraphik ,vol. I, 1898, p. 367, and by G.A. Cooke, A Text-Book of North-Semitic Inscriptions, Oxford, 1903, p. 312, is wholly unacceptable; cf. also J.K. Stark, Personal Names in Palmyrene Inscriptions, Oxford, 1971, p. 111B, simply states: "RBT. Unexplained." There is, indeed, no need or justification for suggesting the replacement of an original ʾaliph by a long vowel.



d - Müller then discusses the following name saying that the central position of the missing spout of the libation table suggests that only the initial letter of the 4th name is missing and that the letter is most probably g without, however, excluding the possibility of h or ḥ.

Müller fails to point out these three important features, viz

(1) parallelism with the right section of the central section requires only four letters in the left section; thus, only one letter is missing;

(2) the missing letter must be either w or ʿ because any other letter would have left its lower left corner or, in case of h, y, etc., the lower extremity of its shaft;

(3) none of those short dedications of human beings ever spells out the family/clan of his/her origin; thus Müller's translation "den [Gi]rābiter" is unfounded. The letter ʿ, which would give [ʿ]rbyn, is also excluded because the word should read [ʿ]rbytn since rʾbm is the name of ...]mʾl's wife.

I read the inscription ScS 7 as follows:

| right-and side | front | left-hand side |
|---|---|---|
| [..][mʾl/hqny/wdm | rʾbm/ /[w]rby | n/ Symbol bdṯḥmym |

[..]mʾil has dedicated to Waddum [his wife] Raʾbum [and][his son] Rabyân. Symbol By Dât-Ḥimyâm.

4 - ScS 12 (cf. Archäologische..., l.c., p. 189 and pl. 34 d).

The photograph of pl. 34 d is printed upside down!!! - J. Schmidt's following statement is most amazing: it reads as follows:

"Bei den Inschriftenfunden Schm/Samsara 11 (Taf. 34 c) und Schm/Samsara 12 (Taf. 34 d) handelt es sich um vermauerte Spolien, von denen jeweils nur eine Ansichtsfläche sichtbar ist und die infolgedessen als Fundstücke nicht beurteilt werden können."

How is it possible for a true archaeologist to declare unworthy of any museographic description two inscribed stones which he presents as "vermauerte Spolien"? It is incredible. And consequently, the reader does not know what kind of stone they are; no measurements are given and, as usual, no letter height. Even Müller does not improve upon Schmidt's total silence; he slavishly repeats "Vermauerter Stein." Furthermore, in his simplemindedness, Müller considers "sicherlich" the stone as being a fragment of a dedication of human beings as in ScS 2, 4 and 7;and he takes almost three lines to state such a simple opinion. The first two texts, however, do not

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b - Müller claims that the 1st element of the author's theophoric name composed of three letters is "nicht mehr erhalten", viz "...ʾl ...ʾil" (p. 186). Yet, the left section of the third letter m is still clearly visible on the stone; thus, [..][mʾl. Müller also errs when he reads the divine name "wd," an orthograph that does not exist in this series of texts; all of which have wdm; cf. ScS 1/2, 2/1-2, 3/2, 6/2, 10/3-4 and 12; the last m is still clearly on the stone; thus, wdm instead of Müller's erroneous "wd."

c - It is on the question of the name rʾb that Müller really goes overboard.

(1) Here again, South-Arabian rʾbm should be spelled "Riʾāb" because it "erfolgt nach" the Arabic pronounciation. What an empty rubbish repeated all over again!!! And to make it even worst, Müller suggests "Riʾābum."

(2) He claims that the Arabic name "Riʾāb, dessen Bedeutung nach Th. Nöldeke 'Schliessung eines Risses, Herstellung' ist"(p. 186). This is untrue: all Nöldeke speaks of is " رُؤْبَة = palm. rbt" (cf. Beiträge zur semitischen Sprachwissenschaft, Strassburg, 1904, p. 100, l. 22); he does not speak of rʾb at all, and neither Arabic riʾâb nor Palmyrene rbt is South-Arabian rʾbm. There can hardly be any doubt that Th. Nöldeke's explanation of ZDMG, 24 (1870), p. 89, which was endorsed by M. Lidzbarski in Handbuch der nordsemitischen Epigraphik ,vol. I, 1898, p. 367, and by G.A. Cooke, A Text-Book of North-Semitic Inscriptions, Oxford, 1903, p. 312, is wholly unacceptable; cf. also J.K. Stark, Personal Names in Palmyrene Inscrip-



tions, Oxford, 1971, p. 111B, simply states: "RBT. Unexplained." There is, indeed, no need or justification for suggesting the replacement of an original ʾaliph by a long vowel.

d - Müller then discusses the following name saying that the central position of the missing spout of the libation table suggests that only the initial letter of the 4th name is missing and that the letter is most probably g without, however, excluding the possibility of h or ḥ.

Müller fails to point out these three important features, viz

(1) parallelism with the right section of the central section requires only four letters in the left section; thus, only one letter is missing;

(2) the missing letter must be either w or ʿ because any other letter would have left its lower left corner or, in case of h, y, etc., the lower extremity of its shaft;

(3) none of those short dedications of human beings ever spells out the family/clan of his/her origin; thus Müller's translation "den [Gi]rābiter" is unfounded. The letter ʿ, which would give [ʿ]rbyn, is also excluded because the word should read [ʿ]rbytn since rʾbm is the name of ...]mʾl's wife.

I read the inscription ScS 7 as follows:

| right-and side | front | left-hand side |
|---|---|---|
| [..][mʾl/hqny/wdm | rʾbm/ /[w]rby | n/ Symbol bdtḥmym |

[..]mʾil has dedicated to Waddum [his wife] Raʾbum [and][his son] Rabyân. Symbol By Dât-Ḥimyâm.

4 - ScS 12 (cf. Archäologische..., l.c., p. 189 and pl. 34 d).

The photograph of pl. 34 d is printed upside down!!! - J. Schmidt's following statement is most amazing: it reads as follows:

"Bei den Inschriftenfunden Schm/Samsara 11 (Taf. 34 c) und Schm/Samsara 12 (Taf. 34 d) handelt es sich um vermauerte Spolien, von denen jeweils nur eine Ansichtsfläche sichtbar ist und die infolgedessen als Fundstücke nicht beurteilt werden können."

How is it possible for a true archaeologist to declare unworthy of any museographic description two inscribed stones which he presents as "vermauerte Spolien"? It is incredible. And consequently, the reader does not know what kind of stone they are; no measurements are given and, as usual, no letter height. Even Müller does not improve upon Schmidt's total silence; he slavishly repeats "Vermauerter Stein." Furthermore, in his simplemindedness, Müller considers "sicherlich" the stone as being a fragment of a dedication of human beings as in ScS 2, 4 and 7; and he takes almost three lines to state such a simple opinion. The first two texts, however, do not

prove his thesis because, according to him, ScS 12 reads ...]wdm/mhqmm. But, the preceding formula does not exist in the first two references, viz

ScS 2/2 has hqny ʾlkbr; and

ScS 4/1-2 has ...](s)ʳmᶜʳm ...]ₜsshmw

Müller transcribes the personal name of ScS 4/2 as "[r]ʾshmw." The "ʾ" is a misreading for s of which the diacritical appendix is straight vertical. Note also that ʾlkbr (ScS 2/2) as well as ]sshmw (ScS 4/2) are the names of the wives of the respective authors. Of course, those two names belong to unspecified men according to Müller, cf. "(den) Ilkabur" (p. 104) and [(den) Ra]ʾshumu" (p. 105). Finally, not even ScS 7 supports Müller's interpretation of ScS 12.

The 2nd, and longest, part of Müller's commentary, almost 8 1/2 lines, is devoted to rehashing commonplaces about mhqmm. Yet, in spite of his fantasy, he failed to see the obvious, fascinated as he was by the name mhqmm, viz he failed to point out the two most important features of the inscribed stone, viz

(1) no letter or part of letter is engraved on the first 6 mm to the right of wdm, and on the last 12-13mm to the left of mhqmm. Yet, the space between the letters in the text is 2-3 mm; and

(2) the shape of the stone is so rough and so coarsely cut that it could not be a section of a long text; it has to stand alone, and the inscription is complete:

wdm/mhqmm To Waddum Muhqimum [has dedicated (this).]

This text both reverses and abbreviates the ordinary dedicatory formula , but the divine name which happens to be also wdm, as here, already starts the dedicatory text Ja 3222/1 (to be published).

Three additional notes.

— Note to p. 95, above the last paragraph.

c - Finally, the mention of the cubic block CIH 692 is all the more wrongly referred to by Jacques Ryckmans that the inscription manifestly engraved by a non-specialist (cf. the ugly forms of both f and y which, respectively, look like some kind of pointy kerner and an upside-down house), shows a widely open m with the left side as an hyperbola which places the text at least four/five centuries later than the boustrophedon period. Therefore, it takes a deranged mind to refer to such an antiquity in the study of an old libation slab with a boustrophedon text.

— Note to p. 97, below the diagram.

W.W. Müller's translation of CIH 439 in H. von Wissmann, Zur Archäologie..., Istanbul, 1968, p. 57, must be corrected on the basis of my Carnegie Museum..., Pittsburgh, 1976, pp. 73-74.

— Note to p. 105, below the second paragraph.



M. Kropp's servility (cf. below, p. 145) is shwon by his endorsement of Jacques Ryckmans' translation of wldhw/wqnyhw as "his posterity and possessions" (cf. PSAS, 22 [1992], p. 55). Furthermore, he inaugurates his whimsical transcription of the word divider as a simple period!

— — — — —

Word list

ʾlmqh (lunar div.) Turner 1/2
ʾlm (pers.n.) CaTh 7
ʾm (invoc.prepos.) Turner 1/2; cf. also dthmym, ʿttr
bwr (fam.n.) d": ScS 6/2
d- (masc.rel.pron.) cf. bwr, ẓbyt, msmʿm
dthmym (solar fem.div.) Turner 1/2; b": ScS 7
-hw (masc.pers.pron.), cf. wld, qny
-hmy (dual pers.pron.) cf. wld, qny
hqny (v.; qny) Turner 1/1, ScS [5/2],6/2,
wdm (lunar div.) ScS [5/2],6/2, 7, 12
wld (pers.s.) pl. in "hw: Turner 1/1-2; "hmy: (Gl 1524/6)
hywm (pers/n.) Turner 1/1
ẓbyt (temple n.) d": Turner 1/1.
y.tʿ (fam.n.) Turner 1/1
yhqm (pers.n.) ScS 6/1
yfʿn (clan n.) CIH 692
[..]mʾl (pers.n.) ScS 7
mhqmm (pers.n.) ScS (5/1-2), 12
mlhhmw (fem.pers.n.) Turner 1/1
msmʿm (temple n.) d": (ScS 6/2-3)
slmt (pers.n.) CIH 692
smʿ (lunar div.) Turner 1/1,2
ʿmʾns (pers.n.) Turner 1/1
ʿttr (stellar div.): Turner 1/1; b": Turner 1/2
qdqm (pers.n.) Turner 1/1
qwmm (fem.pers.n.) ScS 6/3
qny (pers.s.) in "hw: Turner 1/2; "hmy: [Gl 1424/6-7]
(v.) cf. 4th form hqny
qfqfm (pers.n.) ScS 6/1-2
rʾbm (fem.pers.n.) ScS 7
rbyn (pers.n.) ScS 7

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## VI - Fâwite 1:
## ʿÂgil's tombal inscription
## from Qaryat al-Fâw.

### A - Introductory note on the place name.

As in many other similar cases, the spelling of the name al-Faʾw has changed in the past. When I was working there in January 6–18, 1969 (cf. JaMAR, VII [1973], p. 2), the name was written Qaryat al-Faʾw, which was also the spelling used in Gazetteer, No. 54, Arabian Peninsula, Washington, 1961, p. 336A. However, the commanding officer of the local police camp gave me the spelling of "al-Faw" (cf. JaMAR, l.c., p. 122, note a); this orthograph involving the drop of ʾ was adopted by MaISA, pp. 15, 172, 175 and map at the end of the book, and AnQF. But, the normal spelling of long â is found in Gazetteer - Saudi Arabia, Washington, 1978, p. 262B, and GhSA, p.318B. This spelling, however, was rejected by A.F.L. Beeston (cf. below, p. 122) and W.W. Müller who maintains "al-Faʾw" (cf. FiS, p. 33)!!!

### B - A.R. al-Ansary.

The funerary inscribed tombstone Fâwite 1 was discovered "by the southern entrance" of a tomb during the excavations conducted at Qaryat al-Fâw, as stated by its publisher, the excavator himself, A.R. al-Ansary in AnAJ, pp. 3–11 (in Arabic), especially pp. 6 + 8 (transliteration), 7, no. 1 (facsimile), 8 (translation) and 14A (black-and-white photograph). The discovery was already announced in Sources of the History of Arabia, Abstracts of the Papers, Riyadh, 1978, pp. 61 and 64 (in Arabic) and also in AnMT, p. 16, no. 6 (presentation of the antiquity and paraphrase of a partial translation) and p. 17, top (same black-and-white photograph as above). The author reedited his Arabic translation, to which he added his English rendering and a color photograph in AnQF (1981), respectively, pp. 21A (Arabic section), 20A and 146 (English section); this color photograph is also reproduced on the outsides of the jacket of both AnQF and AnPIA. Note that the title of AnMT does not begin with "dalîl," as given by Jacques Ryckmans (cf. AnPIA, p. 82, note 13, where "al-Maʿraḍ" must be corrected to "al-maʿriḍ"), W.W. Müller (cf. FiS, pp. 31, fn 6 and 7, and 36, where the word "DALĪL" is printed in capital letters as if it were the author's name!), and M. Kropp (cf. PSAS, 22 [1992], p. 62), who reports a testimony on mdqnt being "a kind of niche in the wall ... in the region of El-Oudiane - Djerid - Touzeur - Naphta" (p. 62, fn 22). A letter of April 21, 1993, from the Institut des Belles-Lettres arabes of Tunis informed me that mdqnt with the aforementioned meaning and madfan as "an underground granary" do not exist in that



country. Besides, they are not listed in A. Nicholas, Dictionnaire arabe-français, Tunis, 1938, a dictionary apparently unknown to Kropp! The word "dalîl" is not a part of the title because it is printed well above the title itself and in different color on the front cover, and also because it does not appear in the title printed as running head on pp. 1 and 3. In the same reference of W.W.Müller, his spelling of "Faʾw" must be corrected to Fâw, and that of "al-maʿriḍ" must also be corrected to "am-maʿriḍ" because of his spelling of both "aṯ-ṯānī" and "Ar-Riyāḍ." For the plan of ʿÂgil's tomb and the color photographs of both its entrance and interior, cf. AnQF's whole p. 47, respectively, nos. 5, 7 and 6, 8.

For the inscriptions engraved on the western side of J. Tuwayq, east of Qaryat al-Fâw, cf. JaMAR, IV (1973): Ja 2524-2814. The color photographs of some of those texts are printed in AnQF, viz Ja 2768 a-ah on p. 142 (cf. also pp. 148 and 141 and the jacket) and Ja 2772 c-ac + 2771 in p. 143, no. 7 (left and right, respectively).

Here follow the editor's Arabic transliteration, two Arabic and one English translations.

(١) ع ج ل/ب ن/ه ف ع م/ب ن/ل أ خ ه/ر ب ب ل/ب ن/ه
(٢) ف ع م/ق ب ر/و ل ه و/و ل و ل د ه و/و م
(٣) ر أ ت ه/و ول د ه و/و و ل د/و ل د ه م
(٤) و ن س ى ه م/ح ر ى ر/ذ و آ ل/غ ل و ن/ف
(٥) أ ع ذ ه/ب ك ه ل/و ل ه/و ع ن ر
(٦) أ ش ر ق/م ن/ع ز ز م/و و ن ى م/و
(٧) ش ر ى م/و م ر ت ه ن م/آ ب د م
(٨) ب ن/و ك س م/ع د ك ى/ت م ط
(٩) ر/أ س م ى/د م/و ل آ ر
(١٠) ض/س ع ر

(cf. AnAJ, pp. 6 and 8, Arabic section)

1. ʿIjl bin Hafʿām has built for his brother Rab ʾil-Bin
2. Hafʿām a tomb, for himself and his son
3. And his wife and grandsons, and great grandsons
4. And their lawful free women of āl-Ghalwān
5. And asked for them the protection of Kahl, Lah and ʾAthar-Ashraq
6. To preserve them from all distress and weakness
7. And the wickedness, and their wives likewise, for ever
8. From all losses, otherwise may,
9. Heaven rain blood and the earth
10. Be a blaze.

(cf. AnQF, p. 20A, English section)

(١) عجل بن هفعم بنى لاخيه رب ال بن
(٢) هفعم قبرا ، وله ولولده
(٣) ومرأته واحفاده واحفاد أحفاده
(٤) ونسائهم الحرائر من آل غلوان ،
(٥) فاعاذه بكهل ولاه وعثر -
(٦) أشرق من كل ضيق ووني
(٧) وشر وزوجاتهم أبدا
(٨) من كل خسارة ، والا فلتمطر
(٩) السماء دما والارض
(١٠) سعيرا

(cf. AnQF, p. 21A, Arabic section)

(١) عجل بن هفعم بنى لاخيه ربال بن
(٢) هفعم قبرا وله ولولده
(٣) ومرأته واحفاده واحفاد أحفاده
(٤) ونساءهم الحرائر من آل غلوان
(٥) فاعاذه بكهل ولاه وعثر
(٦) أشرق من كل ضيق ووني
(٧) وشر وزوجاتهم أبدا
(٨) من كل خسارة والا فلتمطر
(٩) السماء دما والارض
(١٠) سعيرا

(cf. AnAJ, p. 8, Arabic section)

(The order of the last three quotations was inverted to suit the setup of p.119).

The two major changes of the revised Arabic text are ربال (1.1), which became رب ال, and a hyphen is placed between ʿtr (1.5) and ʾšrq (1.6).

The facsimile published by the editor of AnAJ, no. 1, is inaccurate for all letters are distorted, and even ʾ of ʾḥ (1.1) is erroneously traced as "s."

The back-and-white photograph of AnAJ, p. 11A, is less legible than the color photograph of AnQF, p. 146, but the left edge of this photograph was recut so that f (1.4) and ṯ (1.8) are fragmentary, and m (1.5) is almost entirely missing.

al-Ansary's translation is difficult to endorse because of the following reasons, viz

L.1: the introduction of a hamza in rbbl is unjustified; and bn after rbbl is not a part of the name;

L.3: although the last word is wldhm in the facsimile, both translations have the personal pronoun in the singular; هم was mistakenly read as هو.

L.4: ḫryr is erroneously treated as an adjective qualifying the preceding word nsyhm;

Ll. 5-6: ʾšrq following ʿtr is not a part of the divine name;

Ll. 6 and 8: kl is gratuitously added to the original text;

L.7: mrthnm is inexplicably translated as "their wives," which should be mrʾthm; cf. mrʾth of ll. 2-3; it is not easy to read مرتهنم as مرأتهم.

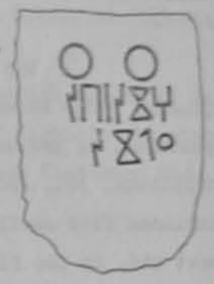


L.8: the translation of wks as "loss, damage" is unacceptable; and

L.10: the letter š is already transcribed as "s" in the transliteration of the text (cf. AnAJ, p. 8). The unfortunate oversight of leaving the letter š incomplete could be responsible for the wrong interpretation of ll. 8-10. The preceding explanation, however, is not confirmed by the treatment of the limestone proscynema of p. 144, no. 7 = Ja 3244; cf. AnQF's facsimile to the right; the three n's are dextrograde. The text may be read as follows:

| | | |
|---|---|---|
| ḥzn/bn | 1 | Ḥazzān, son of |
| ʿlzn | 2 | ʿAlizān. |

Ja 3244



In his reading of the text in both English and Arabic (p. 145 A and B, respectively), ḥ is read "ḫ." The addition of a diacritical sign under ḥ seems to come from the wish of connecting the name ḥzn to the well-known ḫzn, as the root "ḥzn" does not exist in Arabic. Cf. the Safaitic personal name ḥz, e.g., in CIS 1461. Another example of such a distortion is šʿr of Fāwite 1/8 (cf. below). - ʿlzn is known as a feminine personal name in Ja 2760 b/2 (also from Qaryat al-Fāw; cf. JaMAR, l.c., p. 81 and pl. 16).

According to AnQF (p. 28A), the "language" of the authors of the inscriptions "was a blend of southern and northern dialects, and it also had features of northern grammar... the language of the north is clear." These statements must be understood in the light of the overwhelming use of the South-Arabian mimation -m and of the origin of that dialect.

It should be strongly emphasized that AnQF, who publishes the editor's revised translation, expressly lists in his "Selected Bibliography" (p. 30A) A.F.L. Beeston's paper on the inscription (cf. below).

C - A.F.L. Beeston.

Beeston devoted the first half of one of his papers to his study of Fāwite 1 (cf. BSOAS, 42 [1979], pp. 1-2). Here follows his translation which, for the sake of the reader, is here below divided according to the lines of the original inscription:

1 "ʿIgl bin Hafʿam constructed for his brother Habībīl bin
2 Hafʿam the tomb: both for himself and for his child and
3 his wife, and his children and their children's children
4 and womenfolk, free members of the folk Ghalwān. And
5 he has placed it under the protection of Kāhil and Lāh and ʿAththar
6 al-Shāriq from anyone strong or weak, and

7 anyone who would attempt to sell or put (it) in pledge, for all time
8 without any derogation, so long as the sky
9 produces rain or the earth
10 herbage" (p. 1).

Beeston's translation and commentary, the superficiality of which is in accord with its author's notorious pan-classical Arabic fanaticism, will be discussed below. Note that Beeston alone vocalizes "al-Faw" or "Faw."

D - M.A. Bâfaqîh's quadriga.

The book entitled Muḫtârât min an-nuqûš al-yamanîyat al-qadîmat (Tunis, 1985) authored by M.A. Bâfaqîh, A.F.L. Beeston, C. Robin and M. al-Ghûl, contains, as indicated by its title, some Selections ... from South-Arabian inscriptions. Yet, according to p. 473 of "Contents," the list of the chosen texts includes five sections, viz the four dialects of Sabaʾ, Maʿîn, Qatabân and Ḥaḍramawt and, in the fifth place, the sole present text mentioned as "5. Text from Qaryat (al-Fâw)." This title occupies the whole p. 341; and pp. 342-343 present the result of Bâfaqîh's study of the text now listed as "al-Ansary - al-Fâw, no. 1" which is followed by a blank (p. 344).

Not a single word from the inscription is mentioned or referred to in both lists of "words" (pp. 349-412) and "Names" (pp. 413-464). But, the inscription is resurrected in the final list of the book, that of the inscriptions and even in the first place (p. 466). Moreover, the absence of all logic in the book and its numerous irregularities are here illustrated: not a single sample of the well-known Ḥasaean inscriptions is given, but one from Qaryat al-Fâw! Finally, the particularity of the text of not being acknowledged in the last two lists is also shared by two other texts, Ja 986 (p. 334) and 996 (p. 335). Since these three texts did not contribute a single item to those two lists, they should normally be nothing but repetitions of entries already attested somewhere else in the book; in good logic, it was a mistake to waste the equivalent of a little more than five pages on those three texts. Incidentally, the initial name of the Ḥaḍrami king ʾIrym is spelled "ايرم" (p. 335), but "ايرم" in the preceding pages, which is the normal rendering of ʾ of the theophoric names composed with ʾl = ʾil (cf., e.g., pp. 418-419)!

Because of an unknown reason, Bâfaqîh ignores al-Ansary's 1981 reedition of AnQF, which is not even listed in his bibliography. Furthermore, Bâfaqîh reproduces al-Ansary's facsimile without correcting the erroneous "s" to ʾ of ʾḫ (l. 1). And, in spite of claiming to reproduce al-Ansary's interpretation, he nonetheless changed several letters of the original text; e.g., بنى لاخيه رجال of

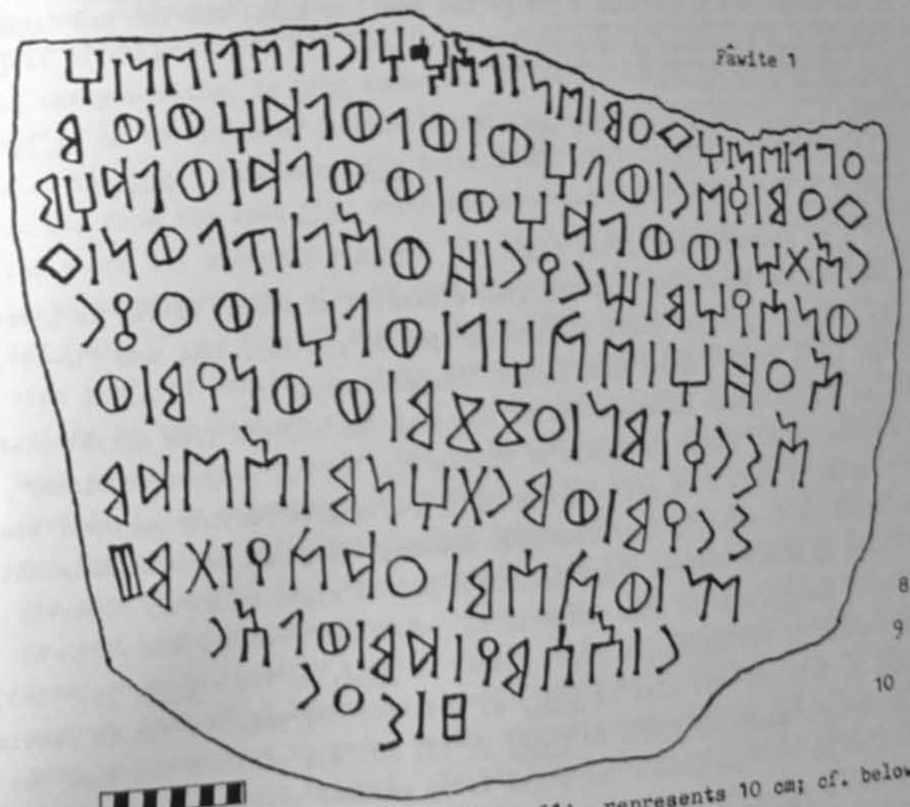


al-Ansary became بنى لاخيه رجال; he also erroneously transfers the last word of l. 1 to the beginning of l. 2; he also erroneously transcribes the family name علوان as غلولن (this, however, could well be an uncorrected printing error). Most importantly, Bâfaqîh gives precedence to al-Ansary's interpretation in a book where Beeston is listed as the 2nd quadriga horse!, and as a consolation price to Beeston, he publishes his Arabic translation of the latter's ll. 9-10 to which he adds an explicative parenthesis according to which, in the dictionaries, "wabara is nqṣ, and šaʿar means nabât" = plants; for these words, cf. below, pp. 142-144.

E - My study.

Here follows my tracing of the black-and-white photograph published in AnAJ; AnQF's color photograph, although more legible, greatly distorts the shape of the letters as if reproducing some kind of squeeze which was flattened and stretched out for the photograph taking.

Fâwite 1



[This handmade scale of AnAJ, p. 11A, represents 10 cm; cf. below, p. 124.]

Then follow my transliteration and translation.

1 ʿgl/bn/hfʿm/bn/lʾẖh/rbbl/bn/h
2 fʿm/qbr/wlhw/wlwldhw/wm
3 rʾth/wwldhw/wwld/wldhm
4 wnsyhm/ḥryr/ḏwʾl/ġlwn/f
5 ʾʿḏh/bkhl/wlh/wʿṯr
6 ʾšrq/mn/ʿzzm/wwnym/w
7 šrym/wmrthnm/ʾbdm
8 bn/wksm/ʿdky/tmṭ
9 r/ʾsmy/dm/wlʾr
10 ḏ/šʿr

1 ʿÂjil, son of Hafʿam, has built for his brother Rabbil, son of Ha-
2 fʿam, [this] tomb and [also] for himself and for his children and his
3 wives and his grandchildren and the children of their descendants,
4 and their women, [all] freeborn, these of the family of Ġalwân. And
5 he has sought the protection of it [= the tomb] by Kahil and Lah and ʿAṯar
6 ʾaš-Šarq from take-over or abandonment or
7 purchase or pawn forever
8 against deterioration until the sky
9 poured rain and the earth
10 vegetation.

1 - The measurements of the stone.

The only precise measurement of any item excavated at Qaryat al-Fâw is given in AnQF, p. 116, no. 1, which shows the photographs of pp. 103, top, and 116-117, to be about 1.28 and 9 times larger than nature, respectively.

There are 20 different handmade scales added to the objects from Qaryat al-Fâw; they are studied below, where the scale of Fâwite 1 is A ( viz proportion width/height of each unit: 2x3) : 10 cm; composed of 10 rectangles resting on their small side, equivalent to 1 cm each, and checkered. The right edge of the extreme right unit is flanked by a very narrow blank delimited on the right by a very close-by vertical line. The purpose of this blank is to point out exactly the width of the last rectangle by isolating its right flank; in AnAJ, p. 11A = AnMT, pp. 16-17, no. 6 = GhSA, p. 323, no. 11; also in GhSA, p. 321, no. 6a, 324, no. 13a (a facsimile of one of the two knobs is found in AnQF, p. 93, no. 4), of which the same two objects are also in no. 13b (where the scale is in reverse position compared with that of no. 13a) = MaJSA, p. 173 (but the negative was printed on the other side); the color photograph of AnQF, p. 93, no. 3 (no scale, and with a ring in each ani-



mal's mouth), and the right head, the same as in AnMT, pp. 14-15, no. 4, shows the same side as GhSA, p. 324, no. 13b. The measurements of the stone with ʿÂjil's text are about 68.75 x 50.9 cm, and the letter height is about 5 cm.

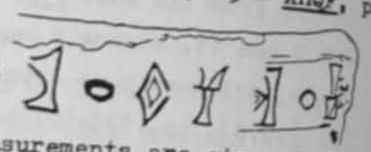
2 - The onomastics.

None of the following valuable points was given by al-Ansary, Beeston , Müller and Robin (cf. below, pp. 126-127); these 3 are interested only in their theories!

The family (cf. ʾl of 1.4) name ġlwn is attested in the South-Arabian gentilic ġlwnyn of Sa 149/2-3; cf. M. Höfner, Sabaeica, Hamburg, 1966, p. 49B and photograph on p. 48, bottom right, where reference is made to Safaitic name ġlwn; cf. my Safaitic Notes, Washington, 1970, pp. 95-96, note 164. Note that G.L. Harding, An Index and Concordance of the Pre-Islamic Arabian Names and Inscriptions, Toronto, 1971, lists the same text LP 969 (= CIS 4381) under both names "ġlwn" (p. 458) and "ġrwn" (p. 454). Notably, the same gentilic word was also found on a tombstone from the same Qaryat al-Fâw in Philby-Qarya 1/4 (cf. G. Ryckmans, in Le Muséon, 62 [1949], p. 88, and photograph in pl. VIII, bottom left). Even more interesting is that the subject of Philby-Qarya 2/1 (cf. G. Ryckmans, l.c.), a text found in the same building as the preceding, is hfʿm according to the photograph of the same pl. VIII (not "yfʿn" as in Ryckmans' reading), and the father of the author of the present text also is hfʿm. This name hfʿm being found exclusively at Qaryat al-Fâw, derived, as it is, from the root fʿm, which is onomastically attested only in Safaitic fʿm of JaS 9 and 11, strongly suggests the identity of those two namesakes. The fourth text to be mentioned here also is very important, Kortler 4 (cf. W.W. Müller in NESE, III [1978], pp. 125-127 and pl. 7, no. 18): another Ġalwânite, who had in his service ywbʾl of Kortler 5, gives the most valuable information which definitively points to the Sabaean origin of the Ġalwânite family or clan, viz he mentions the name of his favored deity, the well-known Sabaean god Ḏû-Sawâwî, and the latter's symbol, ḏ, is reproduced as introducing the text (cf. A. Grohmann, Göttersymbole und Symboltiere auf südarabischen Denkmälern, Vienne, pp. 30-31 and 34). Although the divine symbol is clearly separated from the initial letter of 1.1 and, therefore, had to be understood as a divine symbol, yet Müller still needed Jacques Ryckmans' help to identify it properly!!! As far as the author's name, ġšnm, is concerned, instead of lamenting at not finding an Arabic parallel, Müller would have been well inspired to refer to the Thamudic name ġšnt of JaT 8/1. - The name of the author's father is gnyt. In his usual blinded frenzy to find an Islamic parallel, Müller refers to both a North-African feminine name and Arabic jinnîyat "a female daemon," because, as he demently puts it (p. 126), the genealogical tree of

Bilqîs includes a jinniyat according to some Arabic traditions!!! What does Bilqîs, a mere phantasy, have to do with the author's father? What does an Arabic word extracted from a legend have to do with the author's father? It is already most ridiculous to express such a vue; it is also most unjustified as the inscriptions never mention the name of the author's mother in a simple identity card like this. - Note that Müller erroneously claims that palaeography and onomastics of Sa 149 indicate the text to be Minaean. The palaeography is plain Sabaean, and both names ḫmyt and ḫmym are more frequently attested in Sabaean than in Minaean. Müller failed to see that the mention of dsmwy in Kortler 4 confirms the Sabaean origin of the family/clan ġlwn. Müller's comment in l. 5 (p. 127) on his explanation of two wells being connected with dsmwy based on the etymology of temple names is illusory (cf. above, p. 87). Finally, Müller claims that "dyġrw" must be restored in l. 6 (p. 127). Such a statement, which is repeated twice, is erroneous. Of the initial letter of the line there still remains the upper square top of a letter such as b or ḏ; thus, the letter d is discarded. Furthermore, almost the upper half of the letter r should still be visible. Yet, there is no trace of letter to the left of ġ.

Finally, the last and most precious link identifying that hfᶜm is his namesake mentioned in a text engraved in both single- and double-line tracing on a "part of a silver cup" found during the excavations of Qaryat al-Fâw, viz Ja 3283 = AnQF, pp. 24A and 91, nos. 7 and 8 (partial photograph with the last letter only):




... l]sᶜd hfᶜm ... to] make Hafᶜam happy. - My tracing of the text is on the right. - No scale, no measurements are given in the edition; length of the text in no.7: 11.5 cm at the level of the upper triangle of s; letter height: 2.3 (d) and 2.5 (m) cm. - The division of the 7 (seven) letters in 2 groups is justified by the space between d and h as 1.9 cm on my enlargement, contrasting with 0.45 and 1.3 cm in sᶜd, and 1.2, 1 and 1.2 cm in hfᶜm; this contrast is much less conspicuous than in Ja 3243 (cf. below). - The editor adopts three readings, viz "Masād Haf'am" (p. 24A), "Sa'd Haf'ām" (p. 91B, caption of nos. 7 and 8) and also "Mas'ad" (p. 143, no. 5); this vocalization alone is referred to by W.W. Müller (cf. above, p. 64). The editor's initial "m" is nowhere to be seen.

3 - The palaeography.

None of those writers ever heeded seriously the very particular palaeography of the text. Of course, Beeston is - as usual - so facsinated by his well-known pan-classical Arabic fanaticism that the palaeography, to which he never paid much attention, is considered so carelessly as to allow him to state that the inscription



is "written in fine monumental South Arabian script" (p. 1). This statement is pure crassiness and typically "Beestonian:" the palaeography of the text, where no two letters are of equal height or of identical tracing, cannot even remotely be compared with the well-balanced, well-proportionate monumental letters. As shown in the following schema, the letter types found in ᶜÂjil's inscription represent two different letterings, viz the ordinary one and the other which is typical of Qaryat al-Fâw, viz the transversal bar of ʾ, b, k and s is sometimes replaced by a short, very wide-angle ⌣ which, however, is reversed in ḫ and h ⌣; ḫ is inexistent; this reversion was absolutely necessary lest the lower vertical bar of the letter be almost inexistent by lack of space [[ cf. note on p. 153. ]]

Sabaean Roach 1 and 2 are a proscynema and an incense burner, respectively, from Qaryat al-Fâw published by C. Robin in D.T. Potts ed., Araby the Blest, Copenhague, 1988, pp. 168-175. - No museographic detail, no measurement of any kind is given in the edition; and the paper is loaded with gratuitous statements originating from the author's well-known pan-classical Arabic fanaticism and copied from the master dreamer A.F.L. Beeston (e.g., "the Arabic orthography of the ending -ah/-at$^{u(n)}$" (p.172)), "dyphtongs and long vowels are only found occasionally in the Qaryat al-Fâw texts" (p.170) and his etymology of "ᶜd" from ᶜdy (p. 170; cf. already above, p. 62, W.W. Müller on the name ḫf), or from his fancy theories (e.g., ᶜbd = "client" (pp. 170 and 171), and "ḫr, pl. ʾḫrr 'noble'" (p. 171) ). — Robin always wants to give the impression of knowing everything; e.g., he still refers (p. 171) to Ja "2102 g" (to be corrected to "2102 g/3"); but, it was republished as Ja 2580 b twenty-one years ago in a publication listed on his p. 175B! — Roach 1 and 2 are composed each of two symmetric lines, 9 letters + 2 word dividers, and 5 letters + 1 word divider, respectively; they belong each exclusively to one lettering only, viz the wide-angle and horizontal forms (cf. the following schema). — Note also that Robin does not even refer once (p.175B) to W.W. Müller's paper of FiS!!!

The reader of Robin's paper goes from one surprise to another, and the last one is his invention of a new tune in his boring jingle. For many years Robin used a dull tune, viz "publication personnelle" after the mention of my "Miscellanees... or some other publications. Now, undoubtedly disgusted - who would not be? - by his own old ditty, he set himself to find another aria ... and he came up with a nosy ritornel, viz "without name of publishing house" (p. 175B). The only appropriate answer to such a remark is both simple and unequivocal: "Cela ne vous regarde pas." It is also Robin's second spectacular about-face. The first was denounced in my Miscellanees..., XIV (1985), pp. 279-280: "'when writing that 'une publication d'inscriptions sans photographie est inutile,' Robin is manifestly dishonest because he does exactly what he condemns" (p. 279) ... and he keeps on renegating one major principle of his 1977 doctoral thesis in his last pamphlet entitled Inventaire des inscriptions sudarabiques, Tome 1 (Academie des Inscriptions et Belles-Lettres), Paris, 1992; my review of this pamphlet will be published later. Robin's second about-face is demonstrated by the 55.1886 entry of his Bibliographie generale systematique (Academie des Inscriptions et Belles-Lettres) (cf. above, pp. 7-8), viz s.

Glaser's Mittheilungen ... Erklärung ..., Prague, 1886, where Robin carefully avoids mentioning "In Selbsverlag ..." Incidentally, Robin's copy of the title contains 3 (three) errors, viz "Müller'schen" instead of "Müllerschen" and "al-Hamdānī's" instead of "al Hamdānī's."

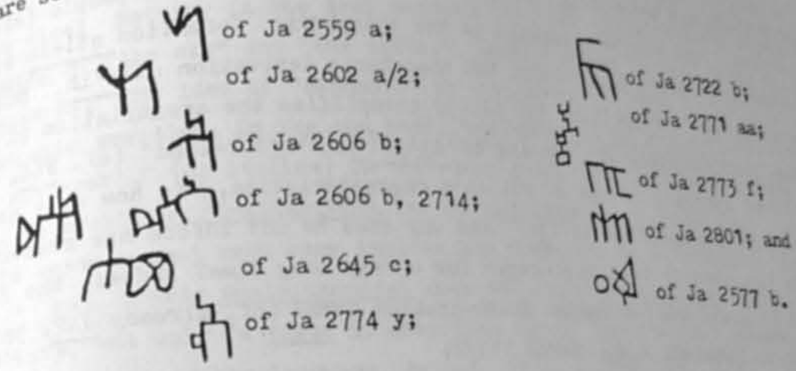
Roach 1: ʾlht/bnt/ʿd ʿbd/bn/ʾftḥ — ʾAlhat, daughter of ʿAwd-ʿabd, son of ʾAftaḥ.

Roach 2: ddn/bn t/ddʾl — Dawduh, daughter of Dawdʾil.

| Fāwite 1 transversal line horizontal | Fāwite 1 transversal line wide-angle | JaMAR, IV (1973) | AnQF and Roach 1 and 2 |
|---|---|---|---|
| | 1.9 | e.g., Ja 2740 h | p. 143, no. 4/1, 2 ; Roach 2/2 |
| | ll. 1,3,4,5, 6,7 | e.g., Ja 2537 g | pp. 144. no. 6 d/1; 147, nos. 2 and 6/1, 2,3; Ja 3241, 3242; Roach 1/1. |
| | ——— | e.g., Ja 2531 b | pp. 143, nos. 4/1, 5; 144, nos. 5/1, 7/1; Ja 3237/1 ; Roach 2/1 |
| | ll. 1,2,5,7 | e.g., Ja 2536 a/1 | pp. 143, no. 2; 144, no. 6 b, d/1; 147, no. 6/1,2,3; Ja 3239 a, b, 3240/2, 3243/1,2; Roach 1/1,2 |
| | l. 8 | (cf. note) | Ja 3242 |
| | ll. 1,2,3,5, 7 | e.g., Ja 2524 b | p. 144, no. 7/1; Ja 3237/1, 2 |
| | ll. 1,3,4 | e.g., Ja 2536 a/1, 2 | p. 147, no. 6/2, 3 ; Roach 1/1 |
| | ——— | e.g., Ja 2556 c/2 | |
| | l. 4 | e.g., Ja 2664 | p. 147, no. 6/2; Ja 3243/2 ; Roach 1/2 |
| | l. 1 | e.g., Ja 2774 o | |
| | ——— | e.g., Ja 2789 b | p. 144, no. 6 c/2 |
| | ——— | e.g., Ja 2771 ay | |
| | ll. 5, 8 | e.g., Ja 2536 c | p. 144, no. 6 d/1; Ja 3240/1,3, 3243/1 |
| | l. 9 | e.g., Ja 2528 | p. 143, no. 5; Ja 3237/2, 3238/1 |
| | ll. 4, 8 | e.g., Ja 2557 | p. 147, no. 6; Ja 3239 a, 3242, 3243/1-2; Roach 1/2 |
| | l. 4 | e.g., Ja 2771 x | |
| | ——— | e.g., Ja 2678 | p. 147, no. 6; Ja 3242 |



Compare several ligatures in the JaMAR, IV, collection, viz.




and other examples published by AnQF, viz

= the divine name khl on a censer (p. 73, no. 5, right) and coins (p. 84), and

on a shard (p. 65, no. 13, upper center), which may be read ʾbg ʾAbag, a monogram already found on a Ḥaḍrami coin.

For the sigla Ja 3237, 3238 and 3239-3243, cf. below.

Furthermore, the palaeography of the inscription is also characterized by

(1) the presence of a serif, either a very short horizontal line or a small triangle, at the end of the staffs and also, quite unexpectedly, at both ends of r, and

(2) large w's, which are a reminder of the Ḥasaean inscriptions.

4 - The four characteristics of the inscription may be presented as follows, viz

a - The writing is phonetic regardless of the actual orthography. This is noticeable in the treatment of the North-Arabic article, ʾal, viz

ʾšrq (1.6) = ʾaš-šarq = South-Arabian šrqn;

ʾsmy (1.9) = ʾas-samī = South-Arabian smyn; and

wlʾrḍ (ll. 9-10) = wa-ʾal-ʾarḍ = South-Arabian wʾrḍn.

W.W. Müller claims that wlh (1.5) is a case identical with "wlʾrḍ, viz. "(wal-Lāh) 'und Allāh'" (cf. FiS, p. 34). As could have been expected, Müller fully endorses the old pan-classical Arabic simplistic opinion of ʾlh = "al-ʾilāh" > "Allāh." It is anachronistic to transpose such evolution to pre-Islamic Thamudic. Furthermore, the abbreviated form lh had its own pronunciation, and the typically Islamic Arabic theory on the waṣlat has no relation with an expression, such as w-lh and

Lah. The god Lah was well-known among the Safaitic and Liḥyanite people (cf. JaaL 8/2 and 61/3-4), and his name appears in the theophoric formation qtl+lh more than 15 times more often than ʾlh in the theophoric derivation qtl+ʾlh. Müller further states that the spelling ʿtr (l. 5) exemplifies the assimilation of tt to tt, thus, corroborating the meaning of ʿattarî as "ʿAttar-land" (cf. FiS, l.c.). This simplistic approach is one of Müller's characteristics, viz how does he know that there is an assimilation in ʿtr? And why does he not inform his reader that the ʿtr form has already been well-known for a very long time? Indeed, the forms ʿtr, ʿtr and ʿtrm are well-attested South-Arabian forms (cf. already JaP, pp. 86-87), viz ʿtr in Minaean, e.g., NaNHB 113/2;

ʿtr in Sabaean, e.g., Ga 52/1, and

in Qat, e.g., Ja 1223/3;

ʿtrm in Qatabanian Ja 1951/3-4.

Even G. Garbini, who spent so much energy and time on the name ʿttr (cf. RSO, 35 [1960], pp. 25-28, and AION, 34 [1970], pp. 409-410) had no remark to offer in his 1971 publication of Ga 52/1. Those secondary forms of the divine name ʿttr must be accepted as such although their justification still eludes us. As far as the noun ʿattarî is concerned, M. Piamenta, Dictionary of Post-Classical Yemeni Arabic, Leiden, 1990-1991, lists it under the root ʿtr (p. 316A; thus reduplication of the 2nd radical, but no assimilation), but also relates it to the Yemeni word ʿantarî (p. 342B), which by the assimilation process became ʿattarî. For this assimilation which includes the letter n, cf. also, e.g., the isolated personal pronouns anta/atta, anti/attî and antu/attû, etc in J. Deboo, Yemenitische Wörterbuch, Wiesbaden, 1989, pp. 18, 19 and 25.

b - The absence of the article after qbr (1.2), ḫryr (1.4), dm (1.9) and šʿr (1.10) follows the Liḥyanite example (cf. JaMAR, VII [1974], p. 101), where the article is omitted "where the object is so well defined or known that it could not be missed or misunderstood." No article accompanies ḫryr because the word obviously refers to all the persons mentioned in ll. 1-4. The article introduces smy and ʾrḍ because the realities indicated by the words are quite unique and could not possibly be either duplicated or undetermined. Similarly, the article does not introduce dm and šʿr because the realities involved are well determined as far as their origin is concerned and are, thus, in no need of determination. Beeston's remarks on his p.1 and fn 1 are exclusively focused on Arabic according to his well-known pan-classical fanaticism. He, however, does not seem to understand the stangeness of his so-called "demonstrative force (implying rendering 'this heaven' and 'this



earth' in 1.9)" as if ʿÂjil had any belief in another sky and another earth somewhere else! Nothing in the text suggests that the author put any emphasis when speaking of "the sky" and "the earth." Beeston seems to be riveted to, and hypnotyzed by, the idea of "emphasis" to such an extent that he fails to see his antagonistic position. On the one hand, "-m has a strongly indefinite force, "any ... whatever'." (my italics) On the other hand, "it would rather seem to correspond to Arabic mā ʾl-ibhāmiyyah (as in rajulun-mā), implying 'any ... whatever." It is certain that each case implies the very same specification, "any ...whatever." But, Arabic mâ-l-ibhâmîyat does NOT imply a "strongly indefinite force" (my italics), but only "a vague intensifying force" (my italics) (cf. W. Wright, A Grammar of the Arabic Language, Beirut, 1974, p. 276, rem. e)!

c - The 4th verbal form is indicated by the prefix ʾ- as in Liḥyanite, Safaitic and Thamudic; cf. ʾʿḍ, 4th form of ʿwḍ (1.5).

d - The most overwhelming feature of the inscription is the mimation, which is used exactly as in South-Arabian: it is added to

five nouns, viz ʿzz (1.6), wny (1.6), šry (1.7), mrthn (l. 7), wks (1.8);

and

one adverb, ʾbd (1.7).

The nature and the circumstances of the realities presented by those words are quite unspecified and undeterminable.

5 - The language.

The four inscriptions referred to above (p. 125) in connection with the identification of the Sabaean family name ġlwn point out to the 1st century B.C.; it is to the same Sabaean origin that belong kdt/wqḥṭn of Ja 635/27 (cf. my Sabaean Inscriptions..., pp. 390-391) as well as qḥṭnyn and qḥṭn/wmdḥg of Fâwite 2/1 and 2, the tombstone of the king mʿwyt/bn/rbʿt (cf. AnAJ, p. 8 and black photograph on pl.11B). The original language of those tribes was Sabaean which, naturally, became the substratum of their late derived dialect. It is, thus, plain irrational as well as erroneous to decide, as Beeston does (cf. l.c., p. 1, fn 1) without having even studied the origin of the author of the text, that there might not be any connection between the mimation of the text and that of the Sabaean inscriptions. Furthermore, although showing the same general palaeographic chacaracteristics, Fâwite 1 and the whole collection of the rock inscriptions of Qaryat al-Fâw (cf. JaMAR, IV [1973]) show a distinct difference of quality, Fâwite 1 being rougher and carelessly done and, thus, exhibiting a less inurement to the writing process. Finally, with his usual narrow-mindedness, W.W. Müller, like a well-trained parrot, whole-

heartedly swallows Beeston's pan-classical Arabic fanaticism and, further, calls Arabic all texts from Qaryat al-Fâw, including the plainly Sabaean Ja 2122, in consequence of the simplistic excuse of the tomb of king m$^{c}$wyt who calls himself qḥ̣nyn the Qaḥ̣ṭânite. But, kdt, qḥ̣n and mdḥ̣m originally were Southerners; and Müller does not know anything else about that kingdom, viz its founder, its evolution and its duration. However, Müller's sheepish position does not come as a surprise; cf. already, JaMAR, XIV, pp. 114-170 for an exposé of his obnoxious plagiaries in Safaitic matters. Taking into consideration the Sabaean substratum, the Sabaean religious tradition (as ascertained by Kortler 4; cf. above, p.125), and the North-Arabian influence, it is rational to call the language of $^{c}$Âjil's text simply Fâwite. Obviously enough, Müller's rehashes will not be taken up below.

6 - The material excavated at Qaryat al-Fâw.

The material excavated at Qaryat al-Fâw witnesses to the general environment in which m$^{c}$wyt's tomb was found, and demonstrates to anyone who is not a blinded pan-classical Arabic fanatic the South-Arabian origin of that culture; but all that information, obviously enough, is passed over in silence without the slightest mention by Beeston, Müller and Robin probably out of deep respect for honest research; see AnQF's documentation on the typically South-Arabian cubic censers incised with the familiar incense names ḥdk (pp. 73, no. 7, left = no. 8, left; 145, no. 9); ḏrw (pp. 73, no. 7, right = no. 8, left center); qsṭ (pp. 73, no. 8, right center; 145, no. 10) and rnd (p. 145, no. 8); the alabaster containers with the well-known formula wd$^{\flat}$b (p. 74, no. 1), the ordinary ustensils (pp. 74-75, nos. 3-5), and in particular the two following inscriptions, Ja 3237, a dedicatory text to the Sabaean deity ḏsmwy (cf. JaP, pp. 131-133) and 3238, a mural text containing two typically Sabaean words, and very specially the very late inscriptions of AnQF, pp. 132-133.

a - Introductory note on Ja 3260 = RÉS 4291 = AO 11.214 BIS.

My study on wd (cf. JaP, pp. 73-74) lists several short names found mostly on seals and amulets, such as $^{\flat}$bwd and ḥ̣mwd. A combination of the two non-divine elements of these two names is attested in Ja 3260, which is another small seal in the form of a rounded, elongated lozenge, of which the width equals 0.46 of its height. My tracing from the photograph is on the right. The characteristics of the four letters are as follows:

(1) the $^{\flat}$ is divided in two horizontal sections;

(2) the two vertical sets of letters are centripetal, viz the left

Ja 3260

half of b is sligthly more developped than the other, as are the right half of the lower section of $^{\flat}$ and the inner half of m as well; the upper horizontal bar of b is protracted toward the center; and the upper horizontal section of $^{\flat}$ is also directed toward the center; and

(3) each vertical half of the name reads from top to bottom.

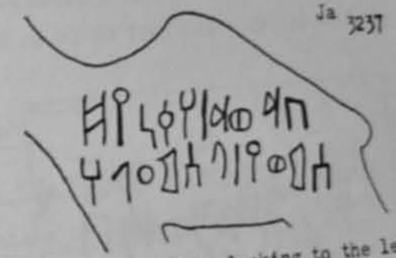
The only unresolved question is to determine where the beginning of the text is, viz is it ḥ̣m$^{\flat}$b or $^{\flat}$bḥ̣m?

The reading ḥ̣m$^{\flat}$b would be supported by the Ḥaḍrami name ḥ̣m$^{\flat}$l of CaTh 23 b/1.

I retain the reading $^{\flat}$bḥ̣m $^{\flat}$Abḥ̣amm because the name is known as a Qatabanian name in Ja 1497.

b - Ja 3237: a dedicatory inscription engraved on the left side of a bronze camel "found in the Temple;" cf. color photograph in AnQF, p. 106, no. 1, left; the camel on the right is reproduced again in no. 2; this pair of camels is also reproduced seven times on the two exterior sides of the jacket and also on the jacket of Pre-Islamic Arabia (1984); cf. my tracing of the inscription on the right.

Ja 3237



(the camel is looking to the left as the animals in the seals/marks from Ethiopia; cf. above, p. 23)

1 bdwd/hqny ḏ

2 smwy/lsm$^{c}$ lh

1 Badwad has dedicated [this] to Ḏû-

2 Samâwî so that He may hearken to him.

For bdwd, a qtwl nominal derivation; cf. Arabic badîd "a like, a fellow" (cf. LaAEL, p. 162A), and the personal names bd in Sabaean Ja 2110/9 and Minaean, e.g., JsaT 506, 354 (cf. JaMAR, IV, p. 140, note 104) is unacceptable because it would imply an unnecessary dissection of the name and a Minaean religious connection which is discarded by the Sabaean divine name himself.

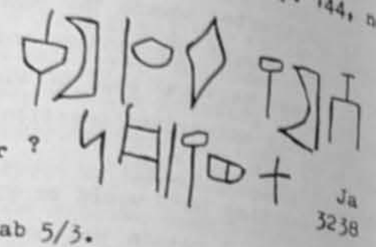
Although engraved by a non-professional, the text nonetheless is composed of two symmetrical lines except for the unavoidable difference of a single letter, viz

9 and 10 letters in ll. 1 and 2, respectively;
1 word divider to isolate the initial word of each line (cf. below, Ja 3243, where the initial name is isolated from the rest by a blank the width of which equals two letters); and
no word divider to isolate the end of each line.

The preceding text proves the existence inside Qaryat al-Fâw of a temple where the Sabaean people or at least their descendants performed their devotions to their Sabaean favored god Ḏû-Samâwî, as already attested in Kortler (cf. above, p. 126). For the absence of the direct complement, cf. above, ScS 5 (p. 111) and 12 (p. 116).

c - Ja 3238: an inscription incised "on a layer of plaster, ... from the sūq," cf. AnQF, p. 132, no. 3, bottom left, with a partial sketch in p. 144, no. 4; cf. my tracing to the right.

| | | |
|---|---|---|
| smyfᶜ/mq | 1 | Sumyafaᶜ, the high |
| twy/dn? | 2 | official of ? |



Ja 3238

What could be more Sabaean that the preceding formula or .../]lwfyhmw in relief on a "part of a copper dish" (cf. AnQF, pp.87, 90, nos. 1 and 2, and 94) "...]for their safety."- mqtwy, pl. mqtt, e.g., Kawkab 5/3.

Kawkab 5 published by G. Ryckmans, in Le Muséon, 66 (1953), pp. 314-315 and pl. II, bottom. - JaSHI (pp. 54-55) has already corrected the editor's distorting interpretation of ll. 3-5 of the text. The following remarks aim at further correcting the editor's misunderstanding of ll. 1-3. My tracing of the inscription is below, and next to it G. Ryckmans' transliteration and translation.

My tracing of Kawkab 5

| | | | | | |
|---|---|---|---|---|---|
| a | 1 | *m'wyt* | *n'mt* | b | |
| a | 2 | *bn/wl't* | *bn/ml* | b | |
| a | 3 | *km/mqtt* | *šrḫ'* | b | |
| | 4 | *l/dyz'n* | | | |
| | 5 | *rbhwd/brḫmnn* | | | |

G. Ryckmans' transliteration (with my alphabetic numerotation added) and translation.

"1 *Mu'âwiyat,*
2 *fils de Wâli'at; Na'âmat,*
3 *fils de Mâlikum, fonctionnaires*
4 *de Šarah'il Dû-Yaz'ân.*
5 *Par le Miséricordieux, Seigneur des Juifs.*"

G. Ryckmans' introduction labors the value of "la ligne sinueuse" dividing the upper three lines into two sections engraved at much lower levels; therefore, he divides those lines most unusually: 2 subjects, each followed by his father's name (1a + 2a and 1b + 2b + 3a, first part), then the official title common to the 4 men (3a, second part + 3b + 4)! The grammatical construction is most fantastic; and it is most surprising and most illogical to see the editor crossing his own so-called dividing line in 2b + 3a and being further forced to repeat the same encroachment in 3a + 3b. It is, thus, immediately obvious that that divisor does not imply, and was not intended to imply, the division of the upper three lines into two different texts. The value of that divisor is to indicate the difference of level between the two sections of each of the three upper lines, which was imposed upon the engraver by the shortage of suitable, necessary space; the same wabble occurs in Ja 1028/12 for the very same reason.

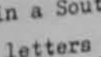


G. Ryckmans also eliminated two word dividers after mᶜwyt (1.1) and rḫmnn (1.5). The end of 1.1 being in the shaded section of the photograph, there is no way of knowing whether there is another word divider at the end of this line.

Noteworthy is that the divisor is not a simple "ligne sinueuse," as described by G. Ryckmans. It is composed of three elements: (a) the upper half is formed by three very wide-angles, ⟩, touching each other and engraved vertically, (b) the bottom of the preceding broken line is connected with a very wide, vertical segment, ⟩; and (c) the bottom of that segment makes contact with a tall, headless, biconvex lens of which the upper left end extends into the right vertical bar of the following b.

Kawkab 5 may thus be read as follows:

| | | | |
|---|---|---|---|
| mᶜwyt/nᶜmt | | 1 | Muᶜawiyat Naᶜamat, |
| bn/wlᶜt | bn/ml | 2 | son of Waliᶜat, son of Mal- |
| km/mqtt | šrḫ' | 3 | kum, high officials of Šaraḥ- |
| l/dyz'n | | 4 | 'il, he of Yaza'ân, |
| rbhwd/brḫmnn/ | | 5 | lord of the Jewry. By Raḥmanân. |

d - AnQF, pp. 132-133. - This is the panel to which Ja 3238 (cf. above, p. 134) belongs. The camel of p. 133, bottom right, is further reproduced in the inset no.2 of the same page. These two pages are very summarily sketched in p. 144, no. 4. But, the camel of the inset no.1 of p. 132 is the same on the right section of pp. 130-131 and, thus, does not belong to pp. 132-133; this is the reason why it is nowhere to be found in p. 144, no. 4. As the author seems to be mainly preoccupied with the "camel hunting" of p. 133, he not only forgot to mention the oryx of p. 132 but also, and very unfortunately, the large inscription incised above Ja 3238 in a South-Arabian lettering belonging to the 6th century A.D.; cf my tracing of the letters ', r and h:  As the central right section of p. 132 is enlarged in p. 129 with the ibex delineated in black paint and another animal also in black paint, it is possible to see some letters better. Unfortunately, the caption of p. 129 does not mention any text. It is to be hoped that this absence of any mention or even of any letter of it on p. 144, no. 4, does not imply that the inscription was not noticed and, therefore, was forgotten. Such a palaeo-

graphy found on a wall of the marketplace of Qaryat al-Fâw is of the utmost value for the historical reconstruction of the site.

As already stated above (p. 132), all preceding information from the excavations of Qaryat al-Fâw is not even mentioned or referred to, thus, is not even considered by W.W. Müller in his formal treatment of the question (cf. FiS, pp. 33-34), where his "Müllerian" prose not only copies A.P.L. Beeston, but he even outdoes him (cf. below). He starts with a general statement, to which he can safely retreat, viz "Aus Qaryat al-Fa'w [sic!] sind einige sabäische Inschriften bekannt" (p. 33). No example, no reference is given, and none can be pinpointed as nobody knows what he has in mind; e.g., on p. 33, Müller lists among his "preislamic Arabic texts" Ja 2122 apparently because its two authors bear "den aus dem nordarabischen Raum wohlbekannten Titel afkal." How ridiculous! Not only it was not the first mention of ʾfkl in South-Arabian - but Müller carefully avoids to spell it out for his reader - although it is an extremely well-known fact that practically is a truism . And ʾfkl is not an Arabic word, but a loanword from Sumero-Accadian, and if the plural ʾfklt is found in a text as old as RÉS 3945/16, what proof does Müller have that it is loanword through North-Arabian? This remark satisfactorily explains Müller's silence on ʾfklt.

Another example of W.W. Müller's workmanship is that, on pp. 32-33 of FiS, he states: "das . dreizeilige Graffito" JsaL 384 is erroneously listed as Liḥyanite because (1) "nfs (nafs) '(Grab)stele'" only means "Seele" in Liḥyanite; and (2) ʾlt of l.2 is the first mention of the Arabic feminine relative pronoun. Müller does not refer to any publication in the footnotes of pp. 32-33; and the two publications which he should have referred to are NOT EVEN listed in his "Bibliographie" of pp. 35-36: "2.2.4.1." Müller should have referred to two books because in fact he not only blindly and slavishly copied, but also attributed to himself every single major statement of W. Caskel, Lihyan und Lihyanish, Cologne, 1954, no. 92, pp. 124-125 and 139B (under nfs) who, in turn, took all his ideas from J.A. Jaussen et M.R. Savignac, Mission archéologique en Arabie (mars-mai 1907), vol. II, Paris, 1914, p. 533 and facsimile in pl. 112. Passing over in silence the triffle (for Caskel, of course) of Caskel combining the four lines of the text into three (an error repeated by Müller), Caskel admits that his interpretation of ʾlt may be a "Fehler?," a question mark to which Müller paid no attention. Furthermore, Caskel fails to translate a section of the second half of the text and, more importantly, he fails (and Müller also follows suit) to check the editors' facsimile on the original stone or on a good photograph. In fact, the facsimile contains one error in l. 3: "b" of



"bnt" is g. Furthermore, Caskel's statements (p. 139) on nfs, sheepishly copied by Müller, are erroneous. Obviously enough, Müller also avoids referring to my Minaean Inscriptions Published as Libyanite, Washington, 1968, for my study of the text conducted on the basis of the study of both the original and a large photograph, an advantage that neither Caskel nor Müller can brag about (pp. 124, note 71, and tracing of the inscription in the annexed plate), where ʾlt is, as in the edition, a second personal name.

7 - The commentary.

L. 1. - ʿgl, cf. Thamudic, e.g., JaT 40b (cf. JaMAR, VII, p. 113 and tracing in pl. 28) and Safaitic, e.g., ClAS 1057 (cf. l.c., XIII, p. 96). The Arabic pronunciation "ʿIgl" is well-known (cf. W. Caskel, Ǧamharat an-nasab, Leiden, 1966, II, p. 353). As Arabic ʿijl means "a one-year old calf" (cf. LaAEL, p. 1964C), I prefer the vocalization ʿajil = ʿâjil "hasting, hastening" (cf. l.c., p. 1964C and 1956C) as more fitting a grown-up ... than "a yearling calf"!

hfʿm, cf. above, p. 125, the study of the author's origin.

bn = bny(?): the apocopated form is also known in Liḥyanite JsaL 384/2 and Sabaean Gl 1720/3 and RoRycBG 1/3. In the present case, however, the haplography with the following word divider cannot be excluded. Beeston notes (p.2) that in the text "long ā remains wholly unmarked in the script, even in word-final position, i.e. the alif maqṣurah, as is shown by bn = banā in the first line." If Beeston refers to more than one case, the reader does not have the slightest idea what he might have in mind and, as usual, he does not even prove the existence of this unique case because he does not even refer to the most obvious interpretation, viz an haplography.

rbbl: the erroneous restitution of ʾ before l (cf. above, pp. 119 and 123) is disproved by the very frequent aphaeresis of ʾ in North-Arabian (Liḥyanite, Safaitic and Thamudic) theophoric names, which is even attested in South-Arabian khnl of Ist 7630/6 and Lu 21/4. Of course, Beeston could not remain silent: he had to say something about it: he notes that "medial -Ī- remains without any notation, certainly in ʿzz and probably in rbbl" (p.2). This remark is manifestly erroneous because rbb of rbbl is easily referred to Arabic rabb "lord, possessor, owner, proprietor" (cf. LaAEL, p. 1003B), a most fitting meaning for a theophoric name. Cf. also the Safaitic personal name rbbly in WH 2509 (cf. JaMAR, IX, pp. 96-97). Finally, Beeston shows again his well-known gratuitous one-sidedness by eliminating without even a mention and/or a justification the Arabic name "RabbĪl" (cf. W. Caskel,

Ḥamḥarat..., l.c., p. 486B), or if he wanted at all cost (just to please his inborn whim) to find an Arabic parallel where the two b's are written separately, why did he not even mention Arabic rabâb "clouds" and ribâb "tithes; oath, promise"? For ʿzz, cf. below.

L. 2. - gbr: Beeston's restoration of "the" before "tomb" in his translation is unjustified because it has no correspondent in the text. His insertion of ":" immediately after "tomb" also is unjustified because wlhw (which follows gbr) does not introduce an explanation or an apposition; it is the second element of a long list which started in l.1 with l²hh.

Ll. 2-3. - wlwldhw/wmr²thw: each of these two words is irrealistically understood by both al-Ansary and Beeston as singular. It is hardly possible that the wealthy ʿÂgil would have remained monogamous and had one child only.

L. 3. - wwldhw: according to Beeston, there is "the appearance of tautology in the repeated wldhw" (p. 2). That tautology exists only in the mind of those who believe that wld means exclusively the immediate progeny of human parents. But everybody knows - with the apparent exception of Beeston who does not even remember Sabaic Dictionary ..., p. 160 - that wld means "child, son, offspring, descendant, progeny," as ²h means "brother, cousin, ally." Furthermore, according to Beeston, "the wldhw of l.3 are that son's children, the grandchildren of Rabībīl" (p.2). There is no tautology in al-Ansary's translation, contrary to Beeston's claim; al-Ansary's English translation makes that point very clear. Moreover, it simply is erroneous to refer, as Beeston does, the pronoun -hw of wwldh to wlwldhw of l.2, thus jumping over the immediately preceding word wmr²th since all personal pronouns h or hw of ll. 1-3 refers to the author ʿÂgil. Of course, those persons are ʿÂgil's grandchildren and, therefore, the children of ʿÂgil's children. But, this does not alter the fact that the unique object of all those personal pronouns h or hw is ʿÂgil, and nobody else.

L. 4. - ḫryr: here again, Beeston's imagination runs wild: he writes that "the alif mamdūdah (-a²) is consistently represented by y; and this seems to be the case even in the middle of a word, if we can take ḫryr as representing ḫarā²ir" (p. 2). Here again, "consistently" presupposes many, at least several, cases; but one only is volunteered. Where are the others? Here again, Beeston gives the impression that ʿÂgil is reproducing in South-Arabian letters a text originally written in Arabic. Such an attitude is grossly anachronic, drastically wrong and nonsensical. The word ḫryr is a normal plural form of ḫrr, next to ²ḫrr; cf. the plural forms qtwl-qtyl, as exemplified, e.g., by dkr/dkwr and hrf/hryf (cf. M. Höfner, Altsüdarabische



Grammatik, Leipzig, 1943, p. 103). The Arabic plural form ḫarâ²ir given by Beeston is feminine, while the masculine plural forms are ²aḫrâr and ḫirâr (cf. LaAEL, p. 538E and also FrLAL, I, respectively, p.360B under feminine ḫurrat, and p.360A under masculine ḫurr). Therefore, without informing the reader, Beeston makes ḫryr the qualificative apposition to nsy (l.4) contrary to the fact that ḫryr qualifies all persons mentioned in ll. 1-4. And to crown his ingenuous, but erroneous analysis, the masculine plural dw (cf. below) would refer to the feminine plural ḫarâ²ir. Here again, Müller (cf. FiS, p. 34) endorses Beeston's anachronistic value of the Arabic ending -ā² which he applies to nsy (l.4), ḫryr (l.4) and smy (l.9)!!!

Ll. 6-7. - Another major innovation introduced by Beeston's translation is his understanding of the four words introduced by mn against (l.6) as designating human doers. This was certainly influenced by South-Arabian phrasaeology. Furthermore, ʿzz and wny were simplistically understood as being antinomical; Beeston could not resist his "instinct" (cf. his Sabaic Grammar, Manchester, 1984, p. 34, fn 61, l. 6; cf. JaMAR, XIV, p. 80) suggesting translating the two words as "strong or weak." It surely takes an extraordinary self-imposed blindness not to see the obvious, viz how ridiculous and insulting to the three deities involved it is to be requested to grant s p e c i a l protection against any "weak" intruder! When studying those two words, Beeston assuredly had in mind the well-known Sabaean antinomy bḥtm/wqṭnm still translated as "great and small" although it means "adult or boy" or, possibly, "master of the house or house servant" (cf. my Carnegie Museum 1974-1975 Yemen Expedition, Pittsburgh, 1976, pp. 95 and 97). Not only does Müller (cf. FiS, p. 34) pocket Beeston's opinion, but he even outdoes him by making his translation even more ridiculous: he translates the expression ʿzzm/wwnym as "ein Starker und ein Schwacher," as if the writer was contemplating two assaults on the tomb, one by a strong man, and the other by a weakling!!! It is, thus, necessary to understand the four words governed by mn as ordinary actions, as al-Ansary had done, and not as the doers performing those actions.

1 - For ʿzz (l.6): it is difficult to understand al-Ansary's translation of ʿzz as Arabic "ḍîq" = narrowness, restriction, limitation," or "distress" as in his English translation. As noted above (p. 138), Beeston understood ʿzz as "certainly" exhibiting the "medial -i-." His certainty is a myth; ʿzz refers to an action, not to an individual. Cf. Arabic ʿazza, the maṣdar of which, ʿazâzat or ʿizz, means "act of overcoming; conquest" (cf. LaAEL, p. 2031C), on the basis of which I translate the noun ʿzz as "take-over."

2 - For wny (l.6): beeston had vocalized wny as "wân$^{in}$" after al-Ansary

who vocalized it as "wan$^{an}$" = weakness. Cf. Arabic wanâ (i), of which one maṣdar is wunîy,"to leave, abandon, cease" (cf. H.A. Salmoné, An Advanced Learner's Arabic-English Dictionary, Beirut, 1972, p. 12391-B, and also A. Cherbonneau, Dictionnaire arabe-français (Langue écrite), Beyrouth, 1973, p. 1405B); therefore, I translate the noun wny as "abandonment."

3 - For šry (l.7): al-Ansary suggested šarr = calamity, disaster, or "wickedness" of his English translation. But, the word is šry, not "šr." Cf. Arabic šir$^{an}$, širâ', šarwat "purchase" and the expression qûwat aš-širâ' "purchasing power" (cf. H. Wehr - J.M. Cowan, A Dictionary of Modern Written Arabic, Wiesbaden, 1979, p. 549A, and H.A. Salmoné, l.c., pp. 404B-405A) and the first meaning of Arabic šarâ (i) is "emit" (cf. FrLAL, II, p. 417A).

4 - For mrthnm (l.7), which is also found in al-Ansary's transliteration of the inscription (p.6), is translated by him as "zawjâtihum" = their wives (also in his English translation). Such an interpretation is unacceptable; the word should have been mr'thm (cf. already above, p. 120). Beeston retains the vocalization of murtahin, the active participle, since he considers the word to be that of a doer. Here again, Müller (cf. FiS, p. 34) swallows Beeston's understanding: he translates mrthnm as "ein Pfandnehmer." But, in fact, it is the word of an action; the vocalization of murtahan must be retained; cf. Arabic murtahan "a thing taken, or received as a pledge"(cf. LaAEL, p. 1173A).

For 'bdm: it is at the end of his remarks on the use of the mimation (cf. above, p. 131) that Beeston writes this: 'bdm "is equivalent in sense to 'for any stretch of time whatever'" (p.1, fn 1). How can one understand that Arabic 'abad$^{an}$, which means "always, forever" = with no time limitation, could under the magic wand of a so-called strongly/vaguely intensive force mean immeasurably less than its original meaning, viz "for any stretch of time whatever," which is necessarily very limited?

L. 8, first half.- Still another major innovation introduced by Beeston's translation deals with bn/wksm, which al-Ansary had translated min kulli hasârati = "from all losses" in his English translation. Of course, "all" does not exist in the original text. To such an interpretation, Beeston objects this:

> "Al-Ansary takes bn as the South Arabian preposition equivalent to Arabic min; but since the latter occurs only two lines before in its normal Arabic form, this seems improbable. Moreover, the placing of the phrase after 'bd suggests that its syntactic status is not parallel with that of the preceding min" (p.2).

(1) Beeston does not justify his gratuitous affirmation that mn (l.6) excludes bn (l.8) as depending also from the verb 'ḫd (l.5). The Arabic preposition mn



is found in l.6 where it belongs to the well-known Arabic expression 'aḫudu BI [+ 3 divine names] MIN [+ 4 nouns], which does not exclude the South-Arabian preposition bn.

Beeston's speciality is the elimination of any information which displeases him. Ll.1-2 of the text provide the first example of an irregular syntax, viz

(a) has built (l.1),
(b) [the INDIRECT COMPLEMENT] for his brother (l.1),
(c) [the DIRECT COMPLEMENT] [this] tomb (l.2), and
(d) [the CONTINUATION OF THE INDIRECT COMPLEMENTS] and [ALSO] for himself, etc.

There is no justification to divide the indirect complements into two sections and to separate the two groups by a direct complement, even extremely short.

Even were the mention of l'hh/rbbl/bn/hf'm before lhw to indicate or suggest that rbbl had already died - but how can the reader be sure of it? -, the phrasaeology would still remain awkward.

Ll. 5-8 present the second example of irregular syntax:

(a) 'ḫd (l.5) (b) bi (l.5) + 3 divine names,
(c) min (l.6) + 4 nouns,
(d) 'bdm (l.7),
(e) bn/wksm (l.8), and
(f) 'dky (l.8) + 2 verbal sentences.

The two sections (d) and (e) should be reversed. Therefore, as usual, Beeston's dreamy argumentation is a wishful thinking contradicted by the whole inscription.

To be sure,the best way to introduce a theory is to posit emphatically that such is the meaning of the text, and the rest only serves as an illustration instead of a real proof. So, Beeston oracularly declares that "What seems required here is something meaning 'without'" (p.2). What is the justification of such an interpretation? None is given; the only justification is his preconceived understanding of what the text should mean. All the rest is nothing but empty babbling. But, that topping is going to determine and define what Beeston really had in mind, viz "hence probably [my italics] read it as the equivalent of the classical bayn in its primary sense of 'separation, parting'." Here again, Beeston is illogical because what is only "probably" becomes absolutely certain in his translation, where "without" is not followed by a question mark. But, there is more: the preceding quotation is immediately followed by this explanation: this "zero marking in the orthography ... is very characteristic of Liḥyanite, where we commonly encounter for instance tm = clas-

sical tym, zd = classical zayd, etc." What is the meaning of this statement? Beeston's "classical" means "classical Arabic" ... as fitting for a pan-classical Arabic fanatic. Besides, he unfortunately does not give the name of his favored jinn or hoepoe that revealed to him that the Liḥyanite names composed of tm had to be related to the root tym, not to tmm. The root tmm "to be complete, perfect" (cf. LaAEL, p. 315A) offers fitting names for human beings. The idea of "servant" is well attested as ʿbd (not "tm"), both as a verb or a noun. In South-Arabian onomastics, tm is used slightly less than tym. The situation of zd = zyd is entirely different because the root zdd does not exist, and the root zwd is used very rarely; cf. the verb zwd in Ḥaḍrami Ja 957/4, and the Sabaean personal name zwdn in Ja 1014g. The North-Arabian, both Liḥyanite and Thamudic, characteristic is that zd is used about twice as often as zyd; in South-Arabian, this proportion is reversed. Then, where is the justification of Beeston's "very characteristic of Liḥyanite"?

(2) On the expression bn/wksm (l.8), Beeston's explanation is a patented "Beestonian" trick, which reads as follows: "In the classical lexica wakasa = naqaṣa, so that our phrase is the equivalent of bi-dūni naqṣān" (p.2). The initial statement as such is irrelevant because an Arabic lexicographical particularity cannot be imposed upon another dialect without any positive proof and especially when it belongs to a later date. It also is erroneous because the fact that those two roots have some meanings in common does not make them synonymous.

Arabic wakasa means "to be defective, deficient;" cf. LaAEL, p. 2964C; cf. also FrLAL, IV, p. 500A, where the root nqṣ is not even mentioned, contrary to Beeston's emphatic statement. Furthermore, as FrLAL, R. Dozy's Supplément aux dictionnaires arabes, Paris, 1967, II, p. 837B, does not mention nqṣ under wks. Beeston's second statement is a vile trick because even were the so-called synonymity of the two roots factual, it could not possibly justify the gratuitous intrusion of bidûni "without" which is Beeston's intended rendering of bn. But, then, the question must be raised, viz where did he get the idea of "bidûni"? He got it from Sabaic Dictionary..., p. 29, — but he could not refer to it since he denied al-Ansary the recourse to South-Arabian — where "to the exclusion of" is the last English entry of bn, and "to the exclusion of" is dûna ġayrihi, which became bidûni since he had to translate bn. Note that the English expression "to the exclusion of" is barbarically translated into French as "à l'encontre de" par Jacques Ryckmans for whom "opposition" and "exclusion" are apparently synonymous. Finally, it is on the basis of the meanings of wks listed above that I translate wks as "deterioration."



Ll. 8 (second half) - 10 do not present any special difficulty.

ʿdky: Beeston's commentary does not analyze the composite conjunction, which, needless to point out, does not exist in Arabic and which is South-Arabian ʿd(y) + enclytic ky = until (i.e. with the emphasis being on the end of the action or the last phase of it); cf. the Liḥyanite composite preposition mn ... ʿdky (JsaL 72/6; cf. JaMAR, VII [1974], p. 146, note 155) = South-Arabian bn ... ʿd from ... to; and the Qatabanian conjunction ʿdkm until. Beeston limits himself to stating that "the conjunction has to be taken in the sense of 'as long as' (mā dāma)" or "rayta/rayta mā" = "whilst, during" of which the primary meaning points to the duration of the act. But, why should the situation be as Beeston says? His classical Arabic fanaticism is the only justification because, for him, Arabic expressions are always and infallibly endowed with the magic power of persuasion. Since the text is not classical Arabic, the characteristics of that Arabic have no value whatsoever to explain the present text.

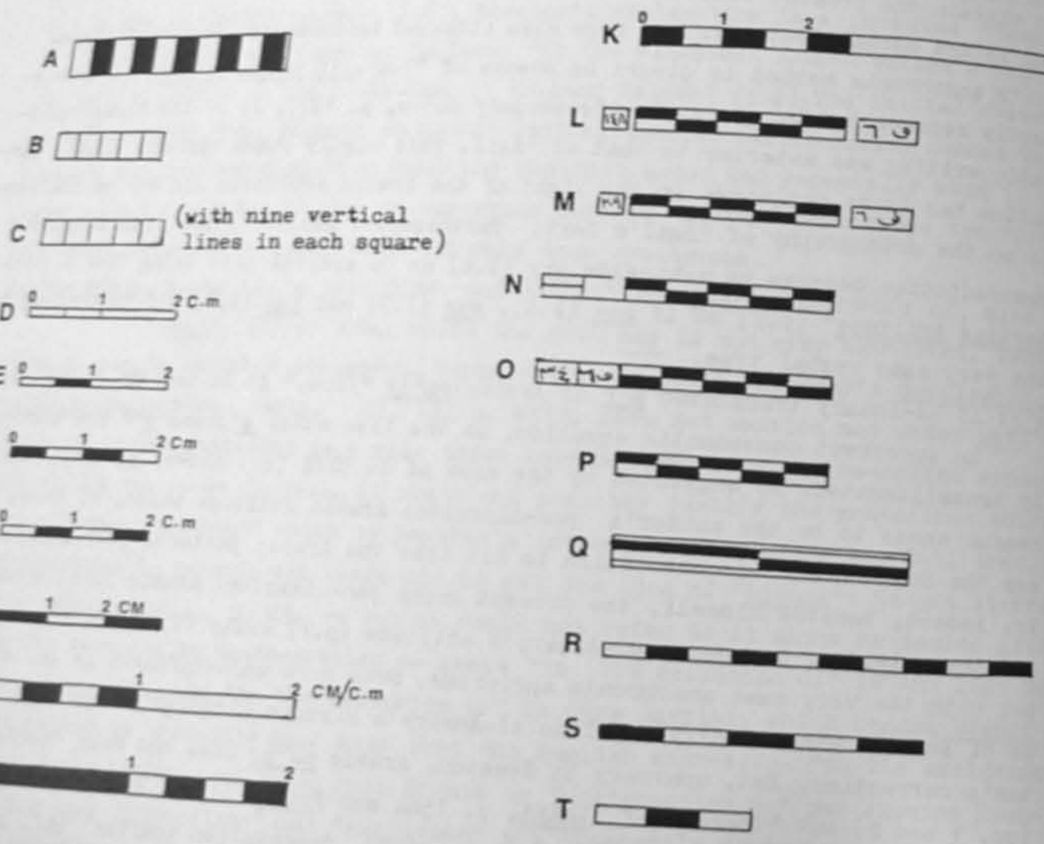
smy is the normal South-Arabian form also attested in Safaitic. Here again, Beeston's anachronic method is clear: he speaks of "the alif mamdūdah (-aʾ) is consistently represented by y" (p. 2, cf. already above, p. 137), as if the classical Arabic writing was anterior to that of ʿÂgil. This highly characteristic Arabic formation has to be explained in the light of the Arabic evolution and has no influence on the orthography of ʿÂgil's text. Furthermore, Beeston's position implies a contradiction because he sets aside wny (1.6) as "a special case being from a root tertiae infirmae" (p.2) as if nsy (1.4), smy (1.9) and bny (1.1) did not belong to the very same verbal type.

šʿr: if al-Ansary translated šʿr as Arabic saʿîr "fire," it is not the result of an incorrect consonantic equation, as the five other s's and š's are correctly transliterated. As suggested by the case of Ja 3244 (cf. above, p. 121), the reason seems to be the author's pan-classical Arabic attitude which, as here, forces the South-Arabic documentation to fit into the Arabic pattern. Very surprisingly, indeed, Beeston himself, the present grand pan-classical Arabic fanatic master and cheer leader, denounces al-Ansary's attitude (p.2) while filling his whole paper with the very same anachronic apriorism. Even more unjustifiable is the attitude of Bâfaqîh who wilfully reprints al-Ansary's mistake although he had read Beeston's correction. But, contrary to Beeston, Arabic šaʿâr does not mean "herbage" (pp. 1 and 2) but "trees" (cf. LaAEL, p. 156A and FrLAL, II, p. 427B), "vegetation, thicket" (cf. H.A. Salmoné, l.c., p. 409B), "végétation touffue" (cf. A.Chertbonneau, l.c., p. 522A) and "arbres touffus et entrelacés, végétation" (cf. J.B. Belot, al-Farâʾid arabe-français, Beirut, 1971, p. 376A). Beeston confused šʿr

with ʿšb; cf. Arabic ʿušb "herbage" (cf. LaAEL, p. 2050B, and FrLAL, III, p. 160A).

Addition I - The handmade scales of AnQF, etc.

*Here follows a schematic presentation of the 20 different handmade scales found with the photographs of some objects from Qaryat al-Fâw. With the exception of B, C and D, where each unit is blank, all scales are checkered. The 8 scales D-K have a numerotation explicit enough to indicate the exact value of each unit; the scale C also is very precise as each unit is divided into ten equal vertical sections by 9 lines, as in an ordinary ruler. The 5 scales L - P are divided lengthwise in two equal sections each of which is further divided in 5 units; the 10 sections are checkered. The scale Q is unique and divided longitudinally in two sections. The last three scales, R - T, are large and used in the photographs of excavated fields.*






A: Fâwite 1, as indicated above, pp. 123-125.

B (proportion width/height of each unit: 3 x 3.5): a 5 cm checkered scale, similar to A; in AnMT, pp. 18-19, no. 8 and GhSA, p. 321, nos. 4 and 5a; the right object of this no. 5a is AnQF, p. 78, upper right; no scale.

C (proportion width/height of each unit: 0.7 x 0.7): a 5 cm scale, of which each unit is divided in 10 vertical sections as in an ordinary ruler; in AnMT, pp. 20-21, no. 9 (only the upper section is visible), 30-31, no. 14 (the left extremity is still on the photograph), 32-33, no. 16 (the lower half was cut off; the caption of p. 32 speaks of a "small" vessel); GhSA, p. 324, no. 12 (the top and the two extreme right units were cut off) = AnMT, pp. 14-15, no. 5 (without scale) = almost the same as AnQF, p. 105, no. 4 (without scale); also in GhSA, p. 323, no. 10 = inscription Fâwite 3, of which the stone measures about 71 x 22.8 cm, and the letter height ± 4.5 cm. For this inscription, cf. M. Kropp in PSAS, 22 (1992), pp. 55-67 (cf. already above, p. 118), where he rejoices at what he calls GhSA's "fortunate error" of mistakenly substituting a photograph for another. But, the reader finds Kropp himself committing the very same mistake in his paper when substituting one caption for the other in p. 66, where the facsimile no. 3 is unjustifiable, and in no.4, correct "5" to 7, bottom; no.1 of p.65 is printed upside down!!! already in l.1 of his paper, correct "323" to 324, and in p. 62, dele "Dalīl" from the title of al-Ansary's 2nd listed publication; the same error was already made by Jacques Ryckmans and W.W. Müller (cf. already above, p. 118). Kropp's servility is evidenced by his reference to A.F.L. Beeston's dreamy juggling in Le Muséon, 91 (1978), of which p. "197" is claimed to have "proposed convincingly the meaning 'entrance-hall, portico, a sheltered space with a roof before the hall" (p. 61, ll. 20-21). In fact, Beeston speaks ONLY of a "portico or entrance-lobby;" that is all! Furthermore, if Beeston argued his case so "convincingly," Kropp should be honest enough to explain why Sabaic Dictionary..., Beeston's dictionary written with the mealymouthed placet of his fawning servants reprints Beeston's opinion as doubtful only and in second place!!! I maintain the translation of mdqnt as "oratoire" or oratory as already given in Ja 411/4 in 1955, contrary to Sabaic Dictionary (l.c.) who attributes this meaning to M.A. al-Ghul's 1962 paper!

The 7 scales D-G,I-K show the unusual division of 1 cm into 2 (D, E, F, G and K) and even 4 (I and J) units. Those scales represent

2 cm in D, E, F, G, I and J; and

5 cm in K.

Furthermore, the scales D, E, F and G are so close to each other that they must

come from the same original.

D (proportion width/height of a unit: 2.5 x 0.5): 2 cm; not checkered; in AnQF, p. 62, top = GhSA, p. 322, no. 7c. The limestone vessel of AnQF, p. 62, no. 1 (without scale) = GhSA, p. 322, nos. 7a and 7b: the scale made from a section of a ruler in these last two reproductions shows that the handmade scale of no. 7c is the wrong one.

E (proportion width/height of a unit: 2.5 x 0.5): 2 cm; same as D, with the central unit black; in AnQF, p. 63, no. 6; no counterpart photograph to check the scale.

F (proportion width/height: 5 x 1); 2 cm; same as D and E, but here, checkered in black starting from the left; in GhSA, p. 321, no. 5b. This scale is wrong: to match that of no. 5a (cf. above, B), each unit of 5b should be 1 cm, instead of 1/2 cm.

G = F in reverse; in GhSA, p. 321, no. 6b; the comparison with the 10 cm scale of no. 6a (cf. above, A) indicates that here as well as in F = no. 5b, the scale is wrong, and that each unit should be labeled 1 cm, instead of 0.5 cm.

H (proportion width/height of a unit: 4 x 0.5): 3 cm; checkered; in AnQF, pp. 68, no. 1, and 112, no. 1. This is a very unusual scale in the series.

I (proportion width/height: 2 x 1); 2 cm; the left checkered half is divided in 4 sections); in AnQF, p. 69, no. 7 = GhSA, p. 322, no. 8b. The figures remain the same in both scales, but the label is slightly different, viz "CM" in the first and "C.M" in the second. The two tracings concern the two objects of AnQF, p. 69, no. 6 (no scale) = AnMT, pp. 32-33, no. 15 (with scale) and GhSA, p. 322, no. 8a, respectively. The captions in both AnQF, p. 69, nos. 6 and 7, and AnMT, no. 15, indicate the stone vessel to be "large." Yet, in no. 8b, the diameter of the vessel being 5.3 cm and the scale of 1.6 cm for 2 cm, the actual diameter of the vessel would be 6.62 cm ... such a vessel can hardly be called "large." On the other hand, if the value of 5 cm is given to the scales of both AnMT, no. 15, and GhSA, no. 8a, as suggested by the other scales, the diameter of the stone vessel would be 50 cm, and such a vessel can safely be called "large."

J (proportion width/height of a unit: 2.5 x 1): 2 cm; same as in I, but in reverse; in AnQF, p. 73, no. 9.

K (proportion width/height of a unit: 2.5 x 1.5): 5 cm; the left half is divided in 5 checkered sections of 0.5 cm each; in AnQF, p. 73, no. 10.

L (proportion width/height of a unit: 6 x 3): 5 cm; checkered and divided lengthwise in 5 equal sections; rectangular addition on the right gives the siglum of



the stratus, "f 6;" and the rectangular one on the left is the inventory number: "148;" in AnQF, p. 80, no. 5.

M (proportion width/height of a unit : 6 x 2.5): 5 cm; same as L, but the inventory number is "209;" in AnQF, p. 99, no. 5.

N (proportion width/height of a unit: 2 x 1): 5 cm, as in L and M, but the two added units are on the left and their contents are difficult to read. In the unit contiguous to the scale, there seems to be a sign looking like a South-Arabian b; the outside unit is divided in two equal horizontal sections; the upper one is blank; but, here again, the design of the lower half remains unclear; in AnQF, p. 144, no. 2 = the tombstone of Mu'awlyat (same photograph in GhSA, pp. 16-17, no.7; no scale in either photographs) and no. 7 (poor facsimile). On the basis of that scale, the measurements of the stone would be about 82.85 x 23.57 cm with a letter height of 6 - 6.4 cm. M. Kropp guessed the scale to be 10 or 5 cm (cf. above, p. 145).

O (proportion width/height of a unit: 6 x 2): 5 cm; scale itself = L - N in reserse; the two additions are on the left as in N with the same stratus, "f 6," but the inventory number is "34;" in AnQF, p. 145, nos. 8, 9 and 10. On the basis of that scale, the incense burner is about 9.77 cm wide, and the letter height about 4.45 cm.

P (proportion width/height of a unit: 2.5 x 0.5): 5 cm, equals the main central section of L - N and O (in reverse); in AnQF, pp. 143, no. 4 and 147, nos. 5 and 6.

Q (proportion width/height of a unit: 11.5 x 1.5): estimated to be 5 cm. The particularity of A is repeated here, but it affects the two long sides (instead of the right extreme edge) each of which is underlined by a long, very narrow blank resting on a long black line; this blank is aimed, as in A, at making the black edge of the scale sharper. The two wooden combs of AnAQF, p. 101, nos. 6 and 9 = no. 7 (center and right, respectively) = p. 100, no. 3 (second from the right and extreme left, respectively), p. 101, no. 9 is also reproduced in p. 95 = 102, lower right; for the same scale, cf. also pp. 104, no. 1 and 105, no. 2 (no. 3 does not have any scale). The scale cannot equal 2 cm because the objects would then be too small; e.g., the width of the first comb would be about 4.5 cm, which would be much too narrow: how could 25 teeth be cut in about 3 cm of wood? Such fine, thread-thin wooden teeth would not last a single use. With a 5 cm scale, the width of the first comb would be about 11.35 cm; thus, about 16.25 cm for the restored comb; that of the second comb would be 10.4 cm; thus, about 12.8 for the restored comb, and finally the height of the "small bronze statue" of pp. 104-105 would be 34.14 cm.

come from the same original.

D (proportion width/height of a unit: 2.5 x 0.5): 2 cm; not checkered; in AnQF, p. 62, top = GhSA, p. 322, no. 7c. The limestone vessel of AnQF, p. 62, no. 1 (without scale) = GhSA, p. 322, nos. 7a and 7b: the scale made from a section of a ruler in these last two reproductions shows that the handmade scale of no. 7c is the wrong one.

E (proportion width/height of a unit: 2.5 x 0.5): 2 cm; same as D, with the central unit black; in AnQF, p. 63, no. 6; no counterpart photograph to check the scale.

F (proportion width/height: 5 x 1); 2 cm; same as D and E, but here, checkered in black starting from the left; in GhSA, p. 321, no. 5b. This scale is wrong: to match that of no. 5a (cf. above, B), each unit of 5b should be 1 cm, instead of 1/2 cm.

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H (proportion width/height of a unit: 4 x 0.5): 3 cm; checkered; in AnQF, pp. 68, no. 1, and 112, no. 1. This is a very unusual scale in the series.

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J (proportion width/height of a unit: 2.5 x 1): 2 cm; same as in I, but in reverse; in AnQF, p. 73, no. 9.

K (proportion width/height of a unit: 2.5 x 1.5): 5 cm; the left half is divided in 5 checkered sections of 0.5 cm each; in AnQF, p. 73, no. 10.

L (proportion width/height of a unit: 6 x 3): 5 cm; checkered and divided lengthwise in 5 equal sections; rectangular addition on the right gives the siglum of



the stratus, "f 6;" and the rectangular one on the left is the inventory number: "148;" in AnQF, p. 80, no. 5.

M (proportion width/height of a unit : 6 x 2.5): 5 cm; same as L, but the inventory number is "209;" in AnQF, p. 99, no. 5.

N (proportion width/height of a unit: 2 x 1): 5 cm, as in L and M, but the two added units are on the left and their contents are difficult to read. In the unit contiguous to the scale, there seems to be a sign looking like a South-Arabian b; the outside unit is divided in two equal horizontal sections; the upper one is blank; but, here again, the design of the lower half remains unclear; in AnQF, p. 144, no. 2 = the tombstone of Muʿawīyat (same photograph in GhSA, pp. 16-17, no.7; no scale in either photographs) and no. 7 (poor facsimile). On the basis of that scale, the measurements of the stone would be about 82.85 x 23.57 cm with a letter height of 6 - 6.4 cm. M. Kropp guessed the scale to be 10 or 5 cm (cf. above, p. 145).

O (proportion width/height of a unit: 6 x 2): 5 cm; scale itself = L - N in reverse; the two additions are on the left as in N with the same stratus, "f 6," but the inventory number is "34;" in AnQF, p. 145, nos. 8, 9 and 10. On the basis of that scale, the incense burner is about 9.77 cm wide, and the letter height about 4.45 cm.

P (proportion width/height of a unit: 2.5 x 0.5): 5 cm, equals the main central section of L - N and O (in reverse); in AnQF, pp. 143, no. 4 and 147, nos. 5 and 6.

Q (proportion width/height of a unit: 11.5 x 1.5): estimated to be 5 cm. The particularity of A is repeated here, but it affects the two long sides (instead of the right extreme edge) each of which is underlined by a long, very narrow blank resting on a long black line; this blank is aimed, as in A, at making the black edge of the scale sharper. The two wooden combs of AnAQF, p. 101, nos. 6 and 9 = no. 7 (center and right, respectively) = p. 100, no. 3 (second from the right and extreme left, respectively), p. 101, no. 9 is also reproduced in p. 95 = 102, lower right; for the same scale, cf. also pp. 104, no. 1 and 105, no. 2 (no. 3 does not have any scale). The scale cannot equal 2 cm because the objects would then be too small; e.g., the width of the first comb would be about 4.5 cm, which would be much too narrow: how could 25 teeth be cut in about 3 cm of wood? Such fine, thread-thin wooden teeth would not last a single use. With a 5 cm scale, the width of the first comb would be about 11.35 cm; thus, about 16.25 cm for the restored comb; that of the second comb would be 10.4 cm; thus, about 12.8 for the restored comb, and finalht of the "small bronze statue" of pp. 104-105 would be 34.14 cm.

The series R - T is composed of scales made of elongated rectangles lying on one of their long sides and used in the photographs taken in the excavation fields; each unit is estimated to represent 10 cm.

R (proportion width/height of a unit: 5 x 1.5): 1 m; scale composed of 10 checkered sections; in AnQF, pp. 36, no. 1, 53, no. 5, 54, nos. 3, 5 (only 9 units are visible on the photographs) and 55, nos. 11, 12, 13; AnAJ, p. 11C, plate 3 (here again, 9 units are visible on the photograph) = AnMT, pp. 28-29, no. 13 (where the outside edges of the two extreme units [only 9] and the lower 2/3 of the length were cut off) = GhSA, p. 323, no. 9a (same remarks as in AnMT, but here 7 units only are represented); no scale is added to the facsimile of GhSA, p. 323, no. 9b, and AnQF, p. 124, nos. 1 (photograph taken in the museum) and 2 (facsimile identical with that of GhSA).

S (proportion width/height of a unit: 8 x 1.5): 50 cm; scale composed of 5 checkered 10 cm units; in AnQF, pp. 41, no. 3, 43, no. 3, 48, nos 3, 4, 54, nos. 4, 5, and 55, no. 9.

T (proportion width/height of a unit: 3 x 1): 30 cm; scale composed of 3 checkered 10 cm units; in AnQF, p. 35, no. 6.

Note that a 40 cm scale of the R - T type, thus exactly between S and T, is seen in Atlal, 12 (1410/1989), pl. 46, right, a partial photograph of Nabataean JsaN 17, which is erroneously called "The Earliest Dated Arabic Document (A.D. 267)" by both J.F. Healey - G. Rex Smith (pp. 77-84 and a grossly distorting facsimile in pl. 46, left). My review of that joint paper denouncing many errors of method, all kinds of anomalies and biased arguments was finished on June 21, 1993, and will be published in the near future.

Addition II - The huge human representation.

The largest panel of the rock inscriptions of Qaryat al-Fâw contains Ja 2768 a-ah (cf. JaMAR, IV [1973], pp. 86-89 and tracings in pl. 17; and a very aslant color photograph in AnQF, p. 142; cf. already above, p. 119). In the upper section of the panel there is "a standing man, whose left hand is holding a spear and his right hand a throwing-stick: a long sword with a rectangular hilt hangs almost horizontally from his waist; he is wearing some kind of long-tailed Phrygian cap" (cf. JaMAR, l.c., p. 87, introduction to Ja 2768 p), and Ja 2768 s is "the pedigree of the standing man, engraved below his waist" (cf. l.c., p. 88). AnQF's caption of his color photograph states that "the drawing is probably 'Kahl' - 'the wise one'." But, Arabic kahl means "being in the flower of manhood, mature, fully

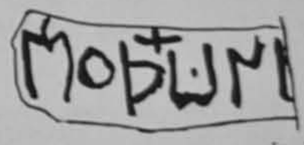


grown man" and kahlat "mature woman, matron" in Wörterbuch der klassischen arabischen Sprache, I, Wiesbaden, 1970, pp. 409A and 411B, respectively; the general meaning of khl as "mature, maturity" is all too clear in pp. 408A-412A ; cf. also FrLAL, IV, p. 67B, and LaAEL, p. 3004C. Furthermore, had the figure been that of the main god, it would have been respected and there would not have been four short Sabaean graffiti engraved on his body. As the engraving of the longest one, Ja 2678 s, is well centered, I take it as the identity card of the man himself. AnQF's caption also speaks of "a spearhead" in the right hand of the figure. But, the top of that weapon is well rounded, not at all pointed.

Addition III - Some Sabaean texts published in AnQF and referred to above, in the right-hand column of p.127.

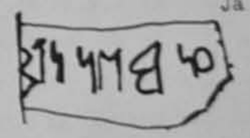
For Ja 3237 and 3238, cf. above, pp. 132-133. - Some texts referred to above were read in the edition. Some others are incompletely known; e.g., p. 143, no. 2: l.2 is illegible on the photograph; and p. 144, no. 6 b (upper center): the initial letter is incomplete.

a - Ja 3239 = AnQF, p. 99, no. 4: photograph printed upside down; two fragments of "camels bones" (p. 99B) with texts painted in red; cf. my tracings to the right.



a

Ja 3239



b

a : the left fragment of a dextrograde inscription; the two b's are upside down:

→ sᶜdt bn bj[...

Saᶜadat, son of B[...[[cf. note on p.154.]]

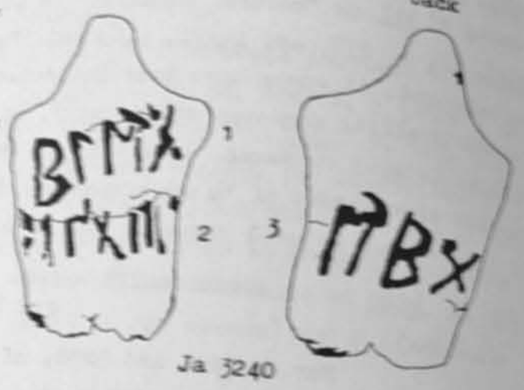
b : right fragment of a sinistrograde inscription:

← ᶜnm bn nyš ᶜAnam, son of Nayš.

ᶜnm, family name in Ja 2737 w (also from Qaryat al-Fâw).-nyš, Thamudic Eut 756. No scale of any kind is seen on the photograph of those two objects.

b - Ja 3240 = AnQF, pp. 118+119: front and back of a crude, massive trunk of a clay woman, limbless and breastless, with a huge neck and large genitalia; wide lines, red and mostly black, decorate face, throat and genitalia; three lines of a dextrograde inscription run across the chest, belly and the lower back, respectively; the letters are irregularly traced with a rounded object before firing and carelessly painted in red after firing; no scale is seen on the two photographs; cf. my partial tracings of both the front and the back on the next page. According to AnQF, the figurine was "found east of the sūq" (p. 26A) and is presented as "a ritualistic" (p. 118, caption) object without any precision or explana-

tion. Yet, the place where the figurine was found is not connected with any "ritual" place of any kind. The editor does not give the measurements of the antiquity, which is presented amids "a quantity of human figurines which appear to have been used as children's dolls in the sūq" (p. 264). But, this antiquity was not found in the sūq, but east of it; and it is rather difficult to imagine that such suggestive a statuette would have ever been used as a doll by children. The statuette could have appropriately served as an advertisement of a lupanar conveniently located in the eastern vicinity of the marketplace.



Ja 3240

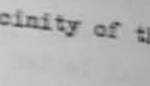
The dextrograde text may be read as follows:

| | | | |
|---|---|---|---|
| → | mlkt | 1 | Malkat, |
| → | bnt/// | 2 | daughter of |
| → | kmt | 3 | Kâmit. |

mlkt, this feminine name is mentioned 4 times in K. Mlaker, Die Hierodulenlisten von Ma'în, Leipzig, 1943, p. 38. - If the 3 vertical lines after bnt have any value beside filling the rest of the line, it remains unknown. The hypothesis of a badly traced, incomplete ʾ (cf. also the incomplete b) is not plausible; the letter would be much too narrow, and the 4-letter name would have been engraved on two sides, which is unacceptable. - kmt, cf. also Qatabanian AM 60.998 or 60.1003 Ac.

c - Ja 3241 = AnQF, p. 144, no.6 a,upper left corner: an incised, sinistrograde "mural inscription;" cf. AnQF's facsimile reproduced on the right:

d²ll Ba²lal.


Ja 3241

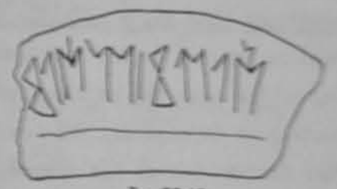
The root d²l is well-known in both North- and South-Arabian. Were the short upper left stroke of the second l missing, the name would read d²ln Ba²lân, which is the name of a man in both Lihyanite JaL 165 and Safaitic CIS 496, and also of a vineyard in Sabaean Ja 1039/2. Here again,the illustration is not accompanied by a scale, and the editor does not give any measurements.— It may be noted here that the rectangular cartouche with an isosceles triangle attached to each angle by its apex, as in p. 144, no. 6 c (center; the right side of no. 6 d on the lower right corner is incomplete) is also attested in Ja 2567, as well



as with the circular cartouche of Ja 2536 b; the use of those isosceles triangles is as varied as fancy; compare the 6 triangles around the square cartouche of Ja 2809 b, also the 8 triangles ornating the 3 letters of Ja 2771 v and the rectangular cartouche of AnQF, p. 144, no. 1, which is further decorated by 6 outside lines ending up in a spiral each. The decorative purpose of those triangles is also attested in the Sabaean calligrams of Ethiopia as well as their ∧ or ∨ appendices of some calligrams (cf., e.g., above, Ja 3264 and 3265 in p. 26).

d - Ja 3242 = AnQF, p. 145, no. 11: an inscription engraved on "a steatite vessel handle;" cf. my tracing on the right.

Here again, no scale accompanies the photograph and no measurements are given in the book.


Ja 3242

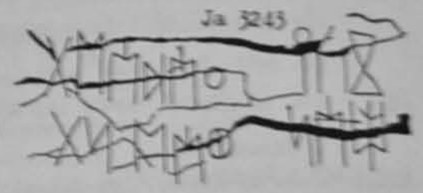
The text may be read as follows:

ʾlǧz/bn slm

ʾAlǧâz, son of Salam.

ʾlǧz, cf. Arabic laǧaza "to distort" and ʾalǧaz "winding, tortuous" (cf. LaASL, p. 2664 B and C); for the ǧ, cf. also above, p.12 in Ja 3263 from Ethiopia. - bn is mentioned above, p. 128. - slm, cf. also AnQF, p. 132, no. 1, upper left corner.

e - Ja 3243 = AnQF, p. 147, no. 7: four words finely incised "on plaster work in the residential area" with more or less horizontal crevices. Those 4 words show the same palaeographical characteristics and form two lines composed of two words each; the 1st word of each line is separated from the balance of the line by an empty space of almost equal length (about 2 letters wide) in each line; cf. above, Ja 3263 (p.126) and 3257 (p.133) where the isolation of the initial word is manifested by a word divider. The engraver balanced the two sections the best he could: 3 + 6 and 3 + 5; and he engraved the letters of the 2nd half of l.2 so that the length of wsbnt almost equals that of ʿbdsbt of l.1. - Cf. my tracing on the right.


Ja 3243

The text may be read as follows:

| | | | |
|---|---|---|---|
| zky | ʿbdsbt | 1 | Zâkî [of the family] ʿAbdsâbit |
| bsn | wsbnt | 2 | treated Wasabnat kindly. |

zky, also painted in black on the wall shown in AnQF, p. 137, where it is the name of the man whose head is shown in pp. 127 = 140, 135 and 136, certainly the master of the house; cf. zky in Safaitic WH 25, and zkyn in Sabaean Ja 2152/1 and Safaitic

CIS 2319; the name of Thamudic HaLi 4, 48, 342 and 371 reads dky Dakī, instead of "zky" of the edition. — ʿbdsbt: for the 2nd component, cf. the Thamudic personal name sbt, e.g., in Hu 485, 93. — ḫsn, cf. Arabic ḥasana, 3rd form "Bene tractavit et egit c. a. p." (cf. FrLAL, I, p. 381A). — wsbnt is an example of the extremely rare qtlnt nominal derivation, as ḫdlnt of Sabaean Ja 616/24, wlnt of Thamudic But 77 a/1, and ʿdnt of Safaitic WH 1297; cf. the personal name wsbm in Qatabanian Ja 1570, and the clan name wsbt in Sabaean Ja 2559 b (also from Qaryat al-Fāw). The 1st name of Safaitic WH 2137 is read "jznt" by F.V. Winnett and G.L. Harding, Inscriptions from Fifty Safaitic Cairns, Toronto, 1978, p. 328, who state that "the root is unknown in Ar." Not only G.L. Harding, An Index... does not list any reading of the name but he also refers the name "bʾdd" to this text (p. 90), where this reading is impossible. This is another proof - in normal circumstances, at least - that Harding had at his disposal a copy different from that which Winnett used. This "jznt" of WH 2137 is a misreading instead of mznt Maznat; cf. mzn, e.g., in Tap 45. —Here again, no scale accompanies the photograph, and no measurements are given in the edition.

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List of Abbreviations


AnAJ = A.R. al-Ansary, "ʾAdwāʾ jadīdat ʿalá dawlat Kindat min hilāl āṯār Qaryat al-Fāw wa-nuqūšuhā," in Sources for the History of Arabia, I (1399/1979) 1981, pp. 6-11, 3 pl. and 1 map.

AnMT = A.R. al-Ansary, al-Maʿriḍ aṯ-ṯānī li-āṯār minṭaqat Qaryat al-Fāw, Riyāḍ, 1977.

AnPIA = A.R. al-Ansary, Pre-Islamic Arabia (Studies in the History of Arabia, II), Riyādh, 1904/1984.

AnQF = A.R. al-Ansary, Qaryat al-Faw. A Portrait of Pre-Islamic Civilisation in Saudi Arabia, Riyadh, 1981.

FiS = W. Fischer ed., Grundriss der arabischen Philologie. I: Sprachwissenschaft, Wiesbaden, 1982.

FrLAL = G.W. Freytag, Lexicon arabico-latinum, Halle, I-IV, 1830-1837.

GhSA = W. Ghoneim, "Saudi-Arabien," in AfO, 27 (1980), pp. 317-324.

JaMAR = A. Jamme, Miscellanées d'ancient arabe, Washington, from 1971 on.

JaP = A. Jamme, "Le panthéon sud-arabe préislamique d'après les sources épigraphiques," in Le Muséon, 60 (1047), pp. 57-147.

JaSHI = A. Jamme, Sabaean and Hasaean Inscriptions from Saudi Arabia, Rome, 1966.

LaAEL = E.W. Lane, An Arabic-English Lexicon, New York, I-VIII, 1863-1893.

MaISA = A.H. Masry, An Introduction to: Saudi Arabian Antiquities, Riyādh, (1975)1977.




Word list : Fāwite 1

ʾl (art. ʾl) cf. smy
ʾḥd (adv.) l. 7
ʾḫ (pers.s.) in 1"h : l. 1
ʾḫ (pers.s.) in dw": l. 4
ʾʿd (v.; ʿd) in 1"h: ll. 4-5
ʾrḍ (s) in 1": ll. 9-10
ʾšrq (div.qualif.) cf. ʿtr
b- (prepos.) cf. khl
bn (v.; = bny) l. 1
-h (remov.prepos.) l. 8
ds (s.) l. 9
dw (pl.masc.pron.) cf. ʾl
-h (masc.pers.pron.) cf. ʾh, ʾʿd, mrʾt
-hw (masc.pers.pron.) in 1": l.2; cf. also wld
-hm (masc.pl.pers.pron.) cf. wld, nsy
hfʿm (pers.n.) ll. 1, 1-2
hks (s.) l. 8
wld (pers.s.) pl.:l.3, in 1"hw: l.2; "hw: l.3; "hm: l.3
wny (s.) l. 6
hryr (pers.s., pl. of hrr) l. 4
knl (div.) in b": l. 5
l- (art., = ʾl) cf. ʾrḍ
(prepos.) cf. ʾh, -hw, wld
lh (div.) l. 5
mṯr (v.) cf. 5th form tmṯr
mn (remov.prepos.) l. 6
mrʾt (fem.s.) in "h: ll. 2-3
mrthn (s.) l. 7
nsy (fem.s.) in "hm: l. 4
smy (s.) in ʾ": l. 9
ʿdky (compos.conj.) l. 8
ʿd (v.; ʿwd) cf. 4th form ʾʿd
ʿgl (pers.n.) l. 1
ʿzz (s.) l. 6
ʿtr (stellar div.) in " ʾšrq: ll. 5-6
ʿlwn (fam.n.) l. 4
ʿl- (conj.) cf. ʾʿd
qbr (s.) l. 2
rbbl (pers.n.) l. 1
śʿr (s.) l. 10
sry (s.) l. 7
tmṯr (v.; mṯr) ll. 8-9

Sabaean

ʾbg (pers.n.) AnQF,p.65,no.13,center
ʾbhm (pers.n.) Ja 3260
ʾḥḏt (fem.pers.n.) Roach 1/1
ʾlgz (pers.n.) Ja 3242
ʾfth (pers.n.) Roach 1/2
b- (prepos.) cf. rhmnn
bṯ (pers.n.) Ja 3239 a
bdwd (pers.n.) Ja 3237/1
bnt (fem.pers.s.) Ja3240/2,Roach1/1, 2/1-2
ddʾl (pers.n.) Roach 2/2
ddh (pers.n.) Roach 2/1
d- (masc.rel.pron.) cf. yzʾn
dʾll (pers.n.) Ja 3241
dnṯ (?) Ja 3238/2
dsmwy (div.) Ja 3237/1-2 in 1": Ja 3237/2
-h (masc.pers.pron.)
hwq (people n.) cf. rb
hzn (pers.n.) Ja 3244/1
-hmw (masc.pl.pers.pron.) cf. wfy
hfʿm (pers.n.) Ja 3283
hqny (v.;qny) Ja 3237/1
wlʿt (pers.n.) Kawkab 5/2
wsbnt (pers.n.) Ja 3243/2
wfy (s.) in 1"hmw: AnQF, p. 87, etc
zky (pers.n.) Ja 3243/1
ḥsn (v.) Ja 3243/2
yzʾn (clan n.) in d": Kawkab 5/4
khl (div.) AnQF,p. 73,no.5,right
kmt (pers.n.) Ja 3240/3
l- (conj.) cf. smʿ
(prepos.) cf. -h , wfy
mlkm (pers.n.) Kawkab 5/2-3
mlkt (fem.pers.n.) Ja 3240/1
mʿwyt (pers.n.) in "/nʿmt: Kawkab 5/1
mqtwy (pers.s.) Ja 3238/1-2; pl.: mqtt: Kawkab 5/3
nyś (pers.n.) Ja 3239 b
nʿmt (2nd pers.n.) cf. mʿwyt
slm (pers.n.) Ja 3242
smyfʿ (pers.n.) Ja 3238/1
smʿ (v.) in 1": Ja 3237/2
sʿd (v.) Ja 3283
sʿdt (pers.n.) Ja 3239 a
ʿbdsbt (fam.n.) Ja 3243/1
ʿdʿbd (pers.n.) Roach 1/1-2
ʿlzn (pers.n.) Ja 3244/2
ʿnm (pers.n.) Ja 3239 b
qny (v.) cf. 4th form hqny
rb (pers.s.) in "hwd: Kawkab 5/3
rhmnn (div.) in b": Kawkab 5/3
šrḥʾl (pers.n.) Kawkab 5/3-4

Note to p. 127, after the upper paragraph. – The transversal, wide-angle characteristic is also found, angular or curved, in a series of Sabaean texts of the Hamburgisches Museum für Völkerkunde und Geschichte nos. 31.300.1604,1607,1611, 1612,1614 and 1619+1620?, respectively, RÉS 4651, 4674, 4661, 4657, 4054 and 3975.

Note to p. 149, at the end of Ja 3239 a. – The dot in the center of the first b is too well placed to be meaningless: it could indicate the beginning of the word.

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P.S.: The other texts from Qaryat al-Fâw published by A.R. al-Ansary will be dealt with in my following Miscellanées..., XIX, as the material complement of the preceding pp. 118-154.



## VII – Thamudic QalḥatT 1, north of ʿAqaba.

The Thamudic inscription QalḥatT 1 was published as Nabataean by D.F. Graf's paper on "The 'God' of Ḥumayma" in Z.J. Kapera ed., Intertestamental Essays in Honour of Jozef Tadeusk Milik, Krakow, 1992, pp. 67-76, photograph and tracing in pl. III and IV, respectively, between pp. 71 and 72.

The present inscription to which no siglum is given in the edition, is one of the texts of which "most...were in Nabataean Aramaic, but a few others were in Greek and Thamudic" (p. 67). "Most of the graffiti" come from "the immense quarrying area in nearby Jebel Qalkha, just to the west of the settlement" (p. 69), which is called "Ḥumayma", some 60 km north of ʿAqaba" (p. 67). The coordinates of J. Qalḥat are 29° 56' N – 35° 17' E according to Jordan – Gazetteer, Washington, 1971, p. 280B, which lists only "Wâdî al-Humaymah" with "h" instead of "ḥ" (p.169B).

The graffito is declared by its editor to be Nabataean (pp. 69-70), "clear in its writing, but difficult to interpret:

šlm br-tlm ʿbd ʾl-ḫwr Peace, BR-TLM, servant of ʾAL-ḪWR" (p. 70).

The preceding understanding is not supported by the interpretation of ʿbd as the noun "servant." It should be either the verb "to make" or the first element of a composite name; but both suggestions are excluded by the context. The interpretation of ʾl as the Arabic article ʾal- instead of the normal noun ʾil god comes from the pan-classical Arabic fanaticism which is nowadays by far the best and most popular master key ensuring the widest possible audience of admirers. For Geraldine King's dishonest argumentation on ʾḫwr, cf. the note on her paper dealing with the Wadi Judayid Thamudic texts in my discussion to be published. Finally, no parallel is given to corroborate the Nabataean formula šlm br ... or šlm br-...

QalḥatT 1 is a typical Thamudic text written in Nabataean letters:

Thamudic šlm bn ṯlm ʿbd ʾl ḫwr = Nabataean šlm br tlm ʿbd ʾl ḫwr

Sâlim, son of Tâlim, was the servant of ʾIl, [who] went away.

The 3 personal names and the 2 verbs are known in Thamudic; for an asyndetic relative clause in Thamudic, cf. my forthcoming rebuttal of Healey-Smith's study of Nabataean JsaN 17 published in Atlal, XII (1410/1989), pp. 77-84 and pl. 46 A-B (cf. already above, p. 148).

šlm, e.g., Hu 744, and in Ja 3242 (from Qaryat al-Fâw; cf. above, p. 151). — ṯlm e.g., Eut 748 and JsaT 23 ; cf. also ṯlm in a Sabaean graffito from W. Juḥir, Ja 3153 a (to be published) and ṯlmt of JsaT 103 d. Note that "Ptolemy" is found in Safaitic CIAS 508 as tlmṯ (cf. JaMAR, XIII, pp. 63-64). – ʿbd, e.g., Hu 497/3. –

ʾl, JsaT 406 a. - ḫwr, HaLi 490+489/1.

Addendum note on fn 10 of p. 313A of G. King's paper in ADAJ, 32 (1988). - King's note presents a shocking case of distortion of a quotation, plagiarism and palaeographical ineptitude; it may safely be singled out as a very representative example of Geraldine King's nonsensical argumenting method.

(1) King begins with "al-ḫawar, 'the third star ...' (Lane)," which is to be coupled with her statement on "Ar. al-ʾaḫwar, 'the planet Jupiter' (Lane)" (p. 314 A, commentary on KiWJ 2). First, King invented Lane's quote on al-ʾaḫwar because Lane says: "al-ʾaḫwar A certain star: (ṢṬ:) or (Ḳ) Jupiter. (ṢṬ,Ḳ)" (p. 666C, ll. 44-45); thus, the expression "the planet" is not from Lane!!! Secondly, she formally plagiaries E. Littmann; here follow the two pericopes one above the other for immediate visual comparison:

G. King: "al-ʾaḫwar, 'the planet Jupiter' (Lane)" (p. 314A) and

E. Littmann: *ʾAḫwar is the planet Jupiter" in G.L. Harding - E. Littmann, Some Thamudic Inscriptions from the Hashemite Kingdom of Jordan, Leiden, 1952, p.34, commentary on HaLi 323, where the phrase quoted above is followed immediately by this: "Taim-ʾAḫwar 'the servant of Jupiter';" and this phrase, in turn, is further repeated in p. 38, commentary on HaLi 393. It should be noted the volume of G.L. Harding - E. Littmann is most used by King; cf. p. 307 and fn 2.

(2) King's 2nd statement of the same fn 10 is that "the name tmlḫwr also occurs in" HaLi 434. Completely hypnotized by her intent of proving her thesis which implies a Harding-Littmann's reading error, she inevitably and wholeheartedly swallows what Harding had described as "the text seems to be all by one hand" and read the sections as b + a because he apparently wanted a text introduced by the preposition l (cf. G.L. Harding - E. Littmann, l.c., p. 41). But, King reverses the order of the two sections to read a + b and, thus, she may find her "tmlḫwr" and she forcibly applied the pan-classical Arabic fanatical theory to interpret -l- as the Arabic article, an argumentation worthy of being signed by the grand master of that fanaticism, A.F.L. Beeston. All their preceding sayings are nothing but empty babblings. From a palaeographical view point, the very first, most obvious fact is the existence of two texts differenciated from each other by three

HaLi 434

a

b

epigraphical facts:

(1) the size of the letters is larger in the lower ellipsis than in the upper, almost vertical line;

(2) the patina is darker in the upper half than in the lower ellipsis; and

(3) the forms of both b and s are different in each section, viz rounded and angular, respectively, in the upper and lower sections.

Therefore, the two texts demanded by the 3 preceding palaeographical features openly contradict Harding's or King's division. It is quite amazing but, at the same time, indicative of the so-called expertise of both Harding and King that neither of them ever saw, or did not wish to see, the above-listed palaeographical differences. The editor's reading of the letters is correct, and his photograph (cf. l.c., pl. XIX) is reproduced above with my lettering added to it.

The two texts may be read as follows.

a - An almost vertical line in the upper section, with letters smaller and a grayish patina darker than in the lower section:

↓ ws$^{c}$d bnt  And Bânit was happy.

s$^{c}$d, e.g., Eut 404 b. - bnt, e.g., Hu 207.

b - An almost complete ellipsis beginning immediately below the end of text a with letters larger and whiter than in a.

↺ ml ḫwr bn ʾys  Was rich Ḫuwwûr, son of ʾIyâs.

ml, e.g., Hu 162. - ḫwr, e.g., Hu 707/2. - ʾys, e.g., JsaT 708. - Note that the dot seen between wr and b is inside the interlinear space and, therefore, valueless for the text.

(July 2, 1993)

## VIII - W.W. Müller and Ja 669.

The most striking characteristic of the first deity of the South-Arabian triad, the lunar god, is the countless varieties of his geometrical and animal symbols, in sharp contrast with the extremely limited symbols of the two other deities of that triad. Those symbols are almost everywhere, and no two of them are quite alike.

The linear geometrical symbols introducing the dedicatory inscriptions are almost a constant pattern; cf., e.g., A. Grohmann, Göttersymbole und Symboltiere auf südarabischen Denkmälern, Vienne, 1914, pp. 6 foll., and the 70 examples of my Sabaean Inscriptions..., Baltimore, 1962, pl. A-C and E.

The representations of the bucranium introduce yet a new dimension, the bas- and high relief.

The linear representations of the bucranium, also found in boundless varieties, developed into two opposite directions, viz the progressive skeletonization of the design to the ultimate schematization of stick figures (as in the coins), and the fancification of the horns to a maze of intertwists of leaves and twigs.

A rather crude example of delineation is seen on a monolyth proscynema in the upper part and an inscription in the lower, CIH 850 = AO 4561 (cf. my tracing on the right). The bucranium alone is referred to by Y. Calvet and C. Robin in their pamphlet entitled Au royaume de Saba: archéologie du Yémen (Cahiers Musée d'Art et d'Essai, Palais de Tokyo, no. 4), Paris, 1980, p. 14A. Note also the interesting inscription because it clearly shows the basic difference between *g* and *l*, viz the left side of the appendix is aslant, like the grave accent, `, in *l*, but is vertical, | , in *g* of the personal name ʾlmgd (l.1).

CIH 850

At the end of the spectrum, the twisting fancification of the horns is vividly exemplified by the well-known RÉS 3568 = Ist 7611, of which a good photograph is found in A. Grohmann, Arabien, Munich, 1963, pl. XXVI, bottom. Museographic description of RÉS 3568: whitish alabaster; maximum thickness: 10.3 cm; front: 66.2 (top) and 64 (bottom) x 43.2 (right) and 42.8 (left) cm; relief thickness: 0.9 cm; text: 25.5 x 13.3 cm.

W. Dostal's paper (cf. R.L. Bidwell and G.R. Smith, Arabian and Islamic Studies, London, 1983, pp. 196-206, 37 facsimiles in pp. 207-209, and 7 photographs



in pp. 210-213) publishes facsimiles of bucrania so atrocious that they cannot but depict the distorted observation skill of their author; e.g., p. 208, bottom, is supposed (?!) to represent the bucranium of RÉS 3568; p. 207, no. 9, is the shameful bastardization of the bucranium of what the author calls "unknown origin" on p. 205, with reference to W.W. Müller's 1973 paper listed on p. 204, where 3 corrections should be made, viz instead of "Yemen. Expedition," read "Jemen-Expedition," and instead of "orientforschung," read "Orientforschung." For this so-called "unknown origin," cf. already CIASA, I (1977), pp. 225-227: YM 375 (also known as YM 1044), the 2nd text of which, B, is published in my Miscellanées d'ancient arabe, XIV (1985), p. 249.

It is at the end of the above-listed paper that Dostal publishes an "Addendum" of 34 lines divised in two sections.

1 - The first section (ll. 1-25 of pp. 202-203) is taken by W.W. Müller's concoctions on two passages of Ja 669 which read as follows:

Ja 669/13-14: -/wyhṣlnn(14)tny/twrn/bklwnm and

23-25: -/f(24)ʿyʿhqnn/wyhṣlnn/twrn/bkl(25)[ʾ]wn/- (cf. Sabaean Inscriptions ... , p. 174B), and their translation printed on the following pages(p. 175A) reads as follows:

ll. 13-14: , and they would drive two bulls to Kalwânum, - and

ll. 23-25: -, they would dedicate and drive the two bulls to Kal[wā]n.

It all started with A.K. Irvine's paper (cf. BSOAS, 30 [1967], p. 285), where he translates the pericope of ll. 23-25 as follows:

"they would dedicate and make a conciliatory offering of two bulls in KLWN," and in fn 55, Irvine refers the verb form hṣl to "Arabic wassala, 'rem peregit, qua ad Deum proprius accederet c. [ilā] p., eique gratior fieret" (cf. G.W. Freytag, Lexicon arabico-latinum, IV, p. 466A). This etymology is naturally endorsed by Sabaic Dictionary... (p. 164) and J.C. Biella, Dictionary... (pp. 150-151) who, by using the adjective "propitiatory" instead of Irvine's "conciliatory" show their dependance from E.W. Lane, Arabic-English Lexicon, p. 3053C. Those writers also give the 4th form the meaning of the 5th form which is the characteristic of denominative verbal form (cf. also G.W. Freytag, l.c., IV, p. 466B). Unfortunately, they do not justify such an unusual etymology which is well in the line of all pan-classical Arabic fanatics. Furthermore, they do not even see that the propitiatory or conciliatory purpose is the ultimate, constant and necessary goal and reason of all dedication. Thus, their translation implies a manifest tautology.

In his note sent to his new suitor, Müller launches out a series of his well-known

"Müllerian" nonsensical imaginations.

a - It should be emphasized that Müller never refers to Irvine's paper although he copied from him the general interpretation of yhślnn while correcting the latter's interpretation of "make a conciliatory offering" to "offer" (ll. 27 and 28) without the slightest justification. But the tautology is even more evident, viz "they would dedicate and offer"! Finally, Müller carefully fails to point out that, in accord with the edition and contrary to his own thesis (cf. immediately below), Irvine considers klwn(m) as a proper name.

b - Müller first makes ex cathedra two unconditional pronunciamentos, viz my "interpretation does not only make any satisfactory sense, but is also hardly supported by the underlying text" (ll. 11-12). Unfortunately, there is not a shred of evidence presented by Müller to substantiate his verdicts; the only "underlying" fact behind his two claims is, as mes Miscellanées ... stated in 1985 (cf. XIV, p. 137), Müller's autosuggestion "that he is the long-awaited messiah in editing SA texts," viz he convinced himself of being The Grand Germanophone Python; cf. above, pp. 59-64 and 110-116 for a few examples illustrating W.W. Müller's exceptional gift of messing up simple texts. It is thus self-evident that, even before the publication of Sabaic Dictionary..., Müller wanted to get even with the three other partners of the same Sabaic..., who apparently dared to slight his concoction, by proving them and the world how well warranted his own discovery really is.

c - Müller's two Pythonian expectorations in fact are the foundations of his thesis, from which he deduces that "since it [= klwn(m)] is, ... a haparlegomenon [sic!] it cannot be determined definitely what it means" (ll. 13-15). Is it not amazing and contradictory, on the one hand, to apparently have no doubt whatsoever on "the underlying text" (cf. above) and, on the other hand, to apparently have no doubt whatsoever that "it cannot be determined definitely what it [= the only and most essential word of his thesis] means"? Furthermore, the reason set forth by Müller is so manifestly unacceptable that it proves how easily Müller bends the reality to suit his fancy. Finally, Müller fails to give the tiniest sign that the two spellings klwnm and klwn demanded any explanation. Indeed, why should they have? A Great Python does not bother with such petty , worthless details. The liberty of adding or using the final -m is especially illustrated in onomastics and particularly by some divine names; e.g., dqbḏ - dqbḏm, wd - wdm, dt/bᶜdn - dt/bᶜdnm, ᶜm - ᶜmm; etc.

If klwn(m) is considered as a noun, as in Müller's fantasy, the proximity of the two forms could only be explained by the at-first-sight rejectable hypothesis



of an author's mistake by omission.

d - Müller's devious mind draws the following conclusions from his preceding statements: "For that reason it [= klwn(m)] will be left untranslated in the forthcoming Sabaean dictionary"(ll.15-16). This is only half of the truth; the sign "< >" means "obscur" (cf. Sabaic Dictionary..., p. XVIII) and is found more than 200 times in that dictionary; also found are "<>?" (pp. 32, 60) and "<>≠ n.pr.?" (pp. 3, 28, 29, 47, 88, 144 and 145).

The word klwn(m) is not only "untranslated" (as Müller puts it); but the sign "< > " indicates that the 4 signers did not have the slightest idea of its meaning. This is quite incredible because the Sabaic... is loaded not only with interpretations presented as "? ?" = "dubious" (p. XVIII), but also as "≠" = "mutually exclusive interpretations"(cf. l.c.). If those authors so openly confess their incapability of finding a single hypothetical meaning, what proof do they have that the word is a "n" = "noun" (p. XVII) or that it certainly derives from the root "klw" (p. 77)? There is absolutely nothing rational in their reasoning.

e - Then comes Müller's ingenious concoction: "klwn(m), with which the bulls destined for sacrifice are provided" (ll. 17-18). This is Müller's typical way of argumenting. As the basic step of his reasoning or his major premise, he simply posits in general terms and without the slightest proof or reason his conclusion which reads as follows: the word "is perhaps to be connected with the word 'bridegroom', widely spread in South Arabia" (ll. 18-19; my underlining). And Müller refers immediately to "R.B. Serjeant, South Arabian Hunt, London, 1976, 67" as well as to 5 modern South-Arabian dialects. To Serjeant's booklet, add also the reference to "p. 122B." Among those dialects is "Soqoṭri kelān," which comes from W. Leslau, Lexique soqoṭri... (Paris, 1938), p. 219. This book already refers to Ḥaḍrami kulân "nouveau marié" in L.W.C. van den Berg, Le Ḥadhramout et les colonies arabes de l'archipel indien, Batavia, 1886, p. 287, fn 30. This form kulân also exists in Daṯinah (cf. C. Landberg, Glossaire daṯînois, pp. 2582-2583, referred to M. Piamenta, Dictionary of Post-Classical Yemeni Arabic, Leiden, 1990-1991, p.435A). R.B. Serjeant - R. Lewcock, Ṣanᶜāʾ (London, 1983) does not mention the noun kulân (or any other spelling) (p. 582A-B). Serjeant's preceding testimony on the absence of the Arabic term kulân is to be understood in the light of his failure to devote a single section of the book to the "Wedding observances" although the words "wedding," "bride" and "bridegroom" are accompanied in the "General Index" by 10 (p. p.630), 7 (p. 616b) and 1 (p. 616b) references. This lacuna is all the more felt

that permissive Serjeant reserves 7 (seven) and 1/2 columns of the book (pp. 171B-175A) and, in addition, the 20 references listed under 16 entries on p. 626C, to the national scourge, the gāt. It is difficult to understand how a writer supposedly dedicated to factual research could formulate without any comment of any kind a general statement such as the "gāt chewing is associated with that good fellowship that English finds in beer drinking" (pp. 174B-175A). The making of such a deceitful statement originates from an author either ignoring - Which is impossible - or refusing to face the individual, familial and social effects caused by a habit that keeps so many human beings under its tyrannic clutch.

In order to build a bridge between "bridegroom" and the bulls of the text, Müller adds that "the word designates the bridegroom on the wedding-day [sic], when he is adorned and crowned, and its original meaning is most probably 'the crowned one, the one adorned with a wreath" (cf. l.c., ll. 22-24). The great copier Müller in all probability took his inspiration from Geʿez kallala "coronare, coronae instar cingere" (cf. A. Dillmann, Lexicon linguae aethiopicae, 1864, col. 816; cf. already I. Ludorf, Lexicon aethiopico-latinum, 1749, col. 386: "kallala coronavit; takallala coronatus fuit, kallul ornatus, coronatus, ʾaklil corona," and more recently W. Leslau, Comparative Dictionary of Geʿez..., Wiesbaden, 1987, p. 283A-B). Most suggestive, however, is T.M. Johnstone's entry "klōn" under the root kln, but the final note is "cf. also klw" which means "to come, bring home (animals, a wife)" in his Mehri Lexicon and English-Mehri Word-list, London, 1987, p. 209. This last meaning may safely be considered as the basic, original idea that went into the derived kln. Indeed, if all bridegrooms bring their wives home, many of them do not wear any crown on that occasion, as I have seen it myself on at least four occasions in Yemen: the bridegroom was NOT crowned. Müller's translation of bklwn(m) as "with wreaths" is not only unjustified from his viewpoint as he states before (cf. above, my underlining of p.159); his translation could therefore be presented at the very most as a mere probability; instead, it is given as a certainty since there is no question mark accompanying it, and even such possibility is not at all justified by T.M. Johnstone's etymology.

2 - The second section (ll. 29-34 of p. 203) is Dostal's final note. - His first 28 lines reproducing Müller's fantasy are followed by six lines devoted to the herbal decoration on the bull's head.

Dostal refers first to S.D.F. Goitein's paper in ZSWG, 9 (1933), "p. 36" and adds that "the bunch of plants is mentioned there as mashqur 'bouquet of flowers' cf. also Landberg C., Glossaire Datinois [sic] Vol. 3 (1942) p. 2067."

The spelling of "mashqur" does not come from ZSWG, l.c., pp.35-36, but from Landberg,



III, p. 2068 (this page is not mentioned by Dostal) where the word is translated as "Blumenschmuck" (cf. also S.D.F. Goitein, Jemenica, Leiden, 1970, p. 51,no.281) which means "Flower ornament" (not "bouquet of flowers" as stated by Dostal). The word "bouquet" used by Dostal comes from Landberg, l.c., p. 2067 (= his reference) and is the translation of šuqrat. The word mašqūr means "[sprig of] aromatic herb" (cf. M. Piamenta, Dictionary...., p. 261B); it is listed as mašqar "sprig of sweet basil" in R.B. Serjeant - R. Lewcock, Sanʿā', p. 586A.

Dostal's repeated confusion with his references to publications is not surprising at all when one takes pains to check his "Bibliography" (pp. 203-205). There are already so many mistakes in p. 203 alone that a whole page would be necessary to list all of them; e.g., the reader admires particularly "Götter, Symbole" instead of "Göttersymbole" and "Auf" instead of "auf" under A. Grohmann; "der religiösen Weltbilder der frühesten Bodenbauern" instead of "des religiösen Weltbildes der frühesten Bodenbauer" under his own name, Dostal; and "The Land of Sheba" instead of "From the Land of Sheba" and "Jew of Yemen" instead of "Jews of Yemen" under Goitein, where it should be mentioned that a "New and Revised Edition" appeared in 1973; etc, etc.

## IX - The calligram Ja 3282 = RÉS 2690.

The gem Ja 3282, known as RÉS 2690, represents two male ibexes standing on their hind legs, back to back, with their heads turned backward and facing each other and with transversally ridged horns (for dotted horns, cf. Ja 3139, to be published). For the contortive position of the two ibexes, cf. also the cubic votive block of CIH 692 (cf. above, p. 95) where the two ibexes are walking instead of standing. In front of their hindquarters stands an unidentified object, and a calligram is engraved between the backs of the animals in the center of the whole design.

The study of this calligram is particularly loaded with errors in the transcription of past literature.

1 - F. Lajard, author of Recherches sur le culte ... de Vénus en Orient et Occident, Paris, 1837, and Tableau et planches," Paris 1849, is erroneously transcribed as "Layard" by A. Grohmann, Göttersymbole und Symboltiere auf südarabischen Denkmälern, Vienne, 1914, p. 56A (with facsimile in fig. 141), D. Nielsen in Handbuch der altarabischen Altertumskunde..., Copenhague, 1927, p. 175 and Grohmann's facsimile in fig. 76, and Répertoire d'épigraphie sémitique, Paris, V (1928), pp. 43 and 44 (cf. also below, p. 165).

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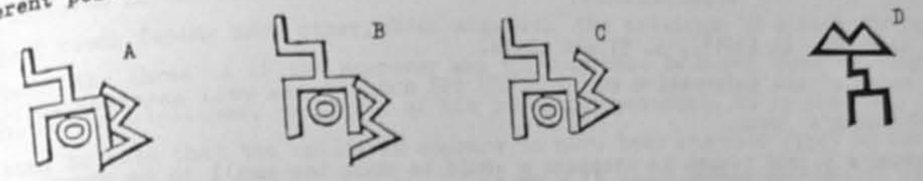
(2) the two extreme aslant lines of m should not end up outside the base of ʾ, but they should, instead, reach the right side of the base of ʾ below its horizontal line.

Here follow the same facsimile of the double-line tracing calligram in three different positions , A - C, and lastly, the m of RÉS 2639 G/6 in D.

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Grohmann's objections are unjustified and irrealistic.

Grohmann's first reason is erroneous because the letter setup in calligrams does definitely not accord necessarily with the direction of the writing. To begin with, some calligrams are found alone. How could they then show the same direction as that of unexisting texts? Cf., e.g., all the calligrams found on jars and ostraca; e.g., from Hajar bin Ḥumeid H 2155+2148: slm, and 3019: khlm. The setup of the letters is planned and regulated above all by the purpose of making the calligram compact, well balanced and distinct; cf., e.g., the calligrams on coins where the letter m is almost constantly dextrograde. Here follow two examples published many years prior to Grohmann's book and repudiating his objection.

a - The letter r is often atop the calligram, thus in vertical position facing up, as in the calligram of mrtdm of CIH 777 (cf. Corpus inscriptionum semiticarum, III, p. 170) where the letter m is dextrograde with a sinistrograde text so as to counterbalance the sinistrograde d on the other side of the vertical line of t. The facsimile was published by J.H. Mordtmann in ZDMG, 30 (1876), p. 293, no. 14, which is reproduced on the right. The position of r is precisely the same as m in fig. D (cf. above).

CIH 777

b - The sinistrograde CIH 393 exemplifies the liberty of engraving because the five

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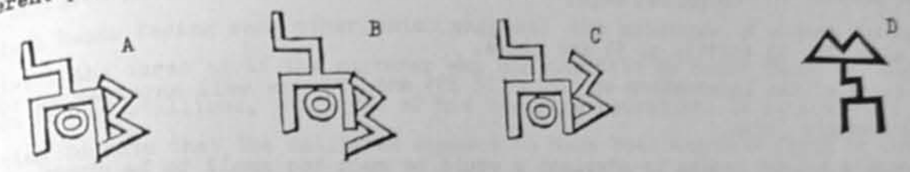


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CIH 777

b – The sinistrograde CIH 393 exemplifies the liberty of engraving because the five

lines of the text contain *five* m's: all of them are dextrograde, as already described explicitely by H. Derenbourg in Les monuments sabéens et himyarites de la Bibliothèque Nationale, Paris, 1891, pp. 27-28:

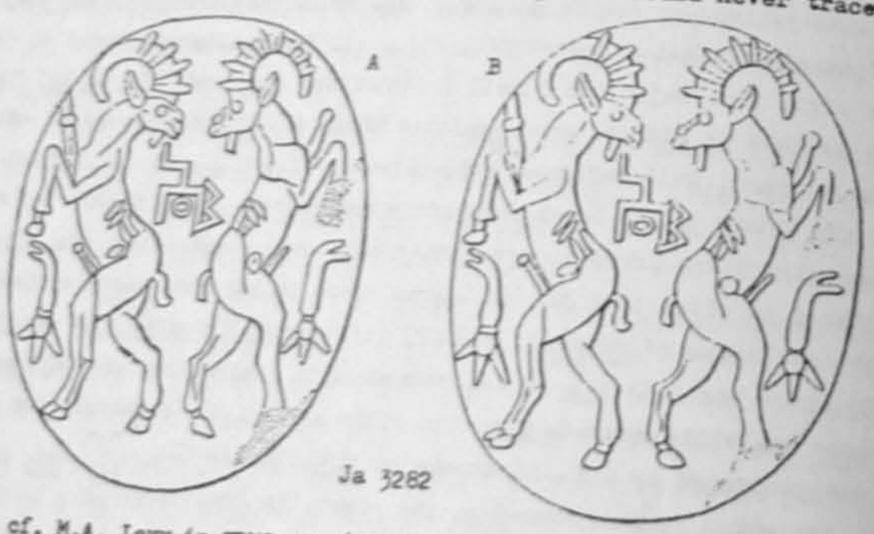
> " *Tandis que l'inscription est écrite entièrement de droite à gauche, les mîm y sont partout formés et inclinés de gauche à droite.* "

Cf. also CIH, II (1911), p. 51 and plate.

The dates of the information on both CIH 393 and 777 are well anterior to 1914 of Grohmann's book.

Grohmann's second reason is utopian: m would be much too small to be drawn. And it does not stand up because its author does not heed the obvious, important fact that the two extreme aslant lines of m touch both extremities of the right vertical line of ʾ; especially the lower end covers the whole width of the ʾ tracing. Were that letter m̃, only the two acute angles might or could be connected with ʾ, but the two extreme ends should remain free of any connection.

As can be seen in figure B of the preceding page, the calligram would be ugly decentralized and manifestly unbalanced. A similar result is seen in C, where the lower extremity of m is attached to the bottom of ʾ. The engraver obviously had no choice whatsoever but to take the solution shown by his engraving because of the shortness of the lower half of ʾ combined with the width of the letter double tracing. B and C show conclusively that the engraver could never trace a normal m with




Ja 3282

A: cf. M.A. Levy in ZDMG, 12 (1858), p. 160.
B: cf. J. Lajard, Recherches..., Tableau et planches, Paris, 1849, p. 11A and facsimile in pl. XXIA, no. 30, lower right corner, second from the right.



the actual width of the double tracing, the height of the lower section of ʾ being much too short. Furthermore, Grohmann fails to take into consideration the beautiful sight of a calligram of which both letters ʾ and m are located below the muzzle of the ibex depicted above and faces the animal, a detail immediately noticeable when looking at the facsimile of the gem (cf. the preceding page). Also, the name engraved in the center of the gem looks very much like a bridle linking the two animal heads facing each other, which suggests the existence of a very strong bond between the three as if the engraver was hinting that he owned these two magnificent horned stallions, the pride of his breeding operation. It is also very interesting to note that the calligram appears to have been engraved first in the middle of the gem because the beard of the right ibex is hanging down vertically as there is plenty of empty space below, but the beard of the left ibex is strongly aslant to the left due to the presence of the upper vertical line of ʾ.

A. Grohmann's reading retained by Répertoire (l.c.) did not find any place in Répertoire..., VIII (1968), either as isolated letters (pp.121A, 215A and 243A, respectively) or as the initials of divine names (pp. 128A, 224B and 245, respectively).

Before proceeding with the palaeographical question, it must be pointed out that M.A. Levy's paper (cf. above, p. 162) is responsible for several errors of information (cf. above, p. 162).

He does not specify that Félix Lajard's Recherches... published in 1837, was followed in 1849 by Recherches..., Tableau et planches, in-4°, II - 12 and 40 pages containing 26 plates; e.g., there are 9 pages numbered "XIV" and "XIV A-H."

Furthermore, Levy's spelling of "Layard" comes from F. Lajard's Recherches..., p. 247, which speaks of "A.H. Layard"'s discoveries in Niniva in 1845.

Levy's worst error, however, is that his facsimile which, although very similar to Lajard's copy, differs in many details, especially along the left side of the gem; the most important discrepancy affects the palaeography of the text and, amongst them, most prominent is the shape of the ʿ inside the bottomless rectangle of ʾ, viz ʿ is strongly elliptical and neatly separated from the long horizontal line of ʾ in Lajard's copy (cf. above, p. 165, B), but it is much more rounded in Levy's facsimile and very closely contiguous to that long horizontal line.

The calligram Ja 3282 contains a minimum of 3 letters, ʾ, m and ʿ, and could contain as many as 7, ʾ, b, ḫ, m, s and ʿ. The letter y is excluded because it does not touch the long central horizontal line of ʾ; this is confirmed by the fact that the lower vertical line of the upper section of ʾ does not cross

the long horizontal line of h. The case of Ja 3260 (cf. above, p. 132) cannot be invoked in an effort to read a possible y. In Ja 3260, the upper section of the left line is unexplicable as such. In the present case, on the contrary, the isolated circle unquestionably is ʿ. The first of several possible names that immediately comes to mind is the well-known

ʾnʿm ʾAnʿam.

Cf., e.g. Liḥyanite JaL 103 c/1, Safaitic Damas 13094 a, Thamudic JaT 104 a, Ḥaḍrami Yatuf 35 a, Minaean Ja 2315, Qatabanian Ja 2449, and Sabaean RÉS 4749, where ʿs1 is certain as in the edition, which also reproduces the final word divider, in E. Mittwoch und H. Schlobies in Orientalia, 6 (1937), p. 309, no. 62, contrary to G. Ryckmans in Répertoire...., VI (1950), p. 348, where š becomes doubtful and the final word divider is eliminated.



X - C. Robin and Ja 3230, 3231 and Mleiḥa 2
(rewritten in June-July 1995).

I - Introduction.

My revision of the preceding pages was almost completed when Raydān, 6 (1994) 1995 was received. This volume is completely under C. Robin's thumb as A.F.L. Beeston's power was totally domineering over the Sabaic Dictionary.

A. Korotayev and especially A. Avanzini renegate their own transliteration system to sheepishly adopt that of their new pacha! As expected, however, A.F.L. Beeston accustomed to lead his flock of the "laudemus invicem" club was not quite ready in pp. 37-39 to endorse Robin's aberrant transliteration gimmicks. The reader is informed that the so-called official name of the Wâdî Bayḥân Museum is given in Italian, French and English by Robin in a French note added to Avanzini's Italian paper, p. 17, fn 1; the museum apparently has NO ARABIC name!? Furthermore, Robin uses the same French note to state that the 4 authors of Avanzini's paper "proposent d'abréger [the name] en MuB."

But, this siglum is never used once in Avanzini's pp. 17-36; "Museo di Bayhān" is repeated in full in almost every page. How useless is that siglum!

The siglum "MuB" suggested by the babblers of the Aden symposium has no connection with the Arabic name because "Mu" of "Museo," "Musée" and "Museum" contrasts with the pronunciation of Arabic "maṯḥaf." The title is normally followed by the number of the antiquity; f.i., 16 (p.17), etc. But, in p. 23, it becomes "Museo di Bayḥā n206." This exception, however, is easily explained by the editor who could not care less of the contents of the paper. But, the siglum is constantly used in the 10 pages of the photos of pp. 148-149 and 151-158! This interrupted sequence of the numbers relating to the Bayḥān Museum = BaM (cf. AM = Aden Museum, YM = Yemen Museum, and BM = British Museum) shows another anomaly of Raydān,6. Here again, the editor could not care less of the contents of Avanzini's paper.

(1) Although the text gives "tav." on each entry of "Museo di Bayhan," the illustrations are indicated as "Pl."
(2) The editor inserted in "Pl. 6" (p. 150) the photo of Ja 2361 which is mentioned without any comment in p. 18 and without any reference to its photo!

II - Preliminary note on A. Avanzini, Robin's new recruit.

A characteristic of A. Avanzini is to haughtily state her opinion without condescending to give a reason to prove it, viz the normal practice of ipsedixitism, and, as if to underline her arrogance, she wilfully isolates her remark from the commentary where it should have been inserted, ... without adding a cross-reference to the place where it should have been written in the first place. She thus selfishly imposes on the

reader the task of finding the right place himself. One example of Avanzini's haughty ipsedixitism is quite enough; she writes in p. 35, last two lines:

"in JA 269 leggere forse hlm e non hbm ; mbs$^2$ da correggere certamente in rb$^2$ in R 4094 e mbrwm in mbrqm in R 3958."

For Ja 269/1 = TC 26/1, cf. my Pièces épigraphiques de Heid bin ʿAqīl ..., Louvain, 1952, p. 130: hbm, and facsimile in p. 245 which shows enough of the two vertical bars of b to make it certain, as confirmed by my photo and my latex squeeze, contrary to Avanzini's "hbm" in pp. 21-22 and 35.

RÉS 4094/6 = KM 48/6: to be honest, Avanzini (cf. also p. 27) should have said that after my study of the original (cf. Le Muséon, 85 [1952], p. 135), I declared b and m of l. 6 certain, as stated in my notebook and confirmed by both my notebook and my paper squeeze.

RÉS 3958/7 = WB 1-14/7 = VL 28/7: mbrdm. Avanzini apparently is not honest enough to tell the reader (cf. also p. 32) that her theory was given by M. Höfner as certain in 1970 from "wahrscheinlich" in 1962; cf. already my Miscellanées ..., I (1971), p. 16, and III (1972), p. 63 and facsimile in pl. 16; both my photos and squeeze confirm this reading.

A final short note on Avanzini. - Fn 22 of p. 26 has no correspondent in the text. She refers to the photo of Ja 2363 in vol. 1 of CIASA's p. 165. Her note is strictly speaking "un cheveu dans la soupe:" absolutely nothing of her p. 26 calls for that footnote.

According to the almost three-millenium year old adage to the effect that "Qui se ressemblent s'assemblent" Avanzini is in good company with C. Robin. P.i., it is quite appropriate to note that, in his report on the 1989 Aden symposium, Robin writes "intention" instead of "attention" (p. 139, last line) of which he apparently is very far from having enough. But, what is much more important and more serious, is that Robin gives a beautiful example of his way of summarizing an historical question by covering up the information ridiculing himself and/or some of his train. I am referring to the 3 antiquities known as Mleiha 1-3, of which his study in pp. 80-82 reveals its author as a coward hiding the truth and a reckless haughty egoist. This last characteristic is also in plain evidence in his study of 3 inscriptions where the testimony of a squeeze has no value since his selfishness and a simple photo are the whole information he wishes to have.

III - Mleiha 3.

Mleiha 3 is a single personal name pecked on the edge of a fragmentary bronze bowl of which a facsimile is published by R. Boucharlat in T. Fahd ed., L'Arabie préislamique et son environnement historique et culturel, Leiden, 1989, pp. 120-121: the author refers to "provisoirement C. Robin, in Sharjah Third Report, p. 77" (cf. fn 26 of p. 120, a report published in 1986) which is "Third Archaeological Surveys and Excavations in the Sharjah Emirate" (cf. fn 5 of p. 112) or "Archaeological Surveys and Excavations in the Sharjah Emirate, 1986: A Third Preliminary Report" (cf. AAE, 1 [1990], p. 102). Cf. below for an enlargement of the facsimile published by Boucharlat without the



name of the drawer of the inscription. This proceiure is probably an indirect way of indicating that he, himself, is the drawer. Were it the case, it would be another example of a text being traced by a non-professional convinced that he could do a good job. Boucharlat also speaks of "piquetage" (p. 120), which simply means "dotted" or "in stippling". But this description is contradicted by the facsimile which is composed of short, solid lines with a few dots in the 3rd, 4th and 5th letters; the first 2 letters and the final word divider are made of solid lines only. For examples of stippling engraving, cf. above pp. 73-78. Therefore, the reader may ask the question: who is responsible? Summarily, research is fed with some material of unknown origin because of the author's self-conceit. For the interpretation of the name, Boucharlat called in J. Pirenne and C. Robin's aid, and the result of this double consultation is worthy of the two consultants who did not deceive the expectation of the reader.

If Pirenne remained silent on the reading of the name, it doubtless is because she declares it to belong to "une écriture proto-arabe" (cf. fn 27 of p. 121). Her stand is quite logical since one pillar of her palaeographical aerial scaffolding is that anything written in non-monumental letters is rejected, thrown away in the proto-Arabic or proto-historic refuse dump. It is indeed a normal reaction to discard and toss away in a waste basket or garbage container anything that is embarassing, contrary or in any way unpleasant.

According to Boucharlat, Robin reads the name as "Mara'shams" (p. 121) without any question mark, a reading that is manifestly disproved by the facsimile.

One year later, M. Mouton, the coauthor of the above-listed Archaeological Surveys ... Third Preliminary Report, mentions the inscribed fragmentary bronze bowl in AAE, 1 (1990), p. 90, with the same reference as Boucharlat (cf. fn 26, p. 120; cf. above). Mouton's mention is insignificant were it not for the new dates given to the fragment (cf. below).

Then, one year later than Mouton's paper and two years after Boucharlat's 1989 article, another essay by the same two coauthors, Boucharlat and Mouton, in PSAS, 21 (1991), reaffirms that "the proper name" (p. 26) "can be read Mara'shams" (cf. fn 10 of p. 27) and, TO PROVE IT, the two coauthors publish A BRAND NEW FACSIMILE of the inscription (p. 33, top), but their illustration comes from the tracing of Boucharlat's 1989 paper. The differences between the two facsimiles of the decorations are very slight; cf., f.i., the design inside the lion tail end and the beads of the circular band as shown in A and C (cf. the next page). But, here again, the sketcher of the new facsimile remains unknown and, most importantly, the two coauthors DO NOT REFER to Boucharlat's 1989 paper! The two facsimiles of the name are so different from each other that NOT

EVEN A SINGLE LETTER IS THE SAME, and ALL LETTERS but one OF EITHER FACSIMILES CANNOT BE FOUND IN THE OTHER. What a consistency! And, the m still encroaches upon the nearby design; but it is now the circular border that should be partly destroyed by the upper extremity of the letter. Of course, in order to hide that obvious fact, the drawer left that extremity of the letter unfinished and incomplete. Finally, what has become of Boucharlat's "piquetage"? (cf. above, p. 171). This is the first time that the facsimile of a text is subreptitiously substituted to another in an attempt to prove the correctness of the initial reading. Congratulations to those members of the Centre National de la Recherche Scientifique for such a trickery!

Boucharlat presents the figure as "une scène de chasse au lion assez particulière; on remarque également la présence d'un cheval" (p. 120). But, this description isolates the horse from the rest although it is a part of the whole, and it does not explain what the author means by "assez particulière." His understanding of the picture as a hunting scene is delusive because one cannot but wonder at the usefulness of cymbals (with the man immediately before the lion), of a dagger (in the left hand of the man behind the animal), and of a horse; all three are useless in a scuffle (?) with a lion. The figure of the bowl looks very much like a jolly panoply of a big-game hunter.

Still according to Boucharlat's testimony, Robin suggests dating "ce document du III$^{e}$ siècle, sur des bases paléographiques" (p. 121). Here again, Robin's palaeographic base, which has never been summarized, cannot but be Pirenne's well-known minichronology. Unfortunately, Pirenne herself ascribes the antiquity to "une date beaucoup plus haute" (p. 121, fn 27), which remains unspecified, and since Pirenne's expression could hardly be vaguer, the reader cannot even fathom the date Pirenne had in mind. And, still according to Boucharlat's testimony, Pirenne refers to P. Boneschi's 1956 and "1958" papers. This date must be corrected to "1953" (cf. RSO, 28 [1953], pp. 105-107). It is quite obvious that Pirenne does not refer to W.F. Albright in BASOR, No. 128, Decembre 1952, pp. 39-45 instead of to Boneschi's useless papers, but her reference to them symptomatizes her up-to-date knowledge of those inscriptions! The state of dejection in which those precious epigraphic relics are held is illustrated by the fact that the length of the inscription is about 1/6 of that of the typological sketch of the same bronze bowl. It is precisely the opposite of what was demanded by the respective importance of each item. Finally, Boucharlat did apparently not realize the irony of the opposition between "une inscription en caractères sud-arabes" of his caption of fig. 4 (p. 121) and an "inscription proto-arabe" of Pirenne in fn 27 of the same page! The last item to be mentioned is the synopsis of the dates given to the fragmentary bowl, which shows a constant trend to reduce it progressively, viz

A = the facsimile released by Boucharlat in 1989, p. 121, and immediately below, the enlargement of the name, which would be read yrnnm/ Yuranmin. For the form of Y, cf. RÉS 2696/2 (cf. my Miscellanees ..., XVI [1968], pp. 298-301), and Arabic ranima, 2nd form "to sing, intone, shout."

B = A with large dots added to the 5 original letters so that they may correspond to Robin's reading. The 2nd triangle of the initial m is added below the 1st as demanded by the axis of the letters. Needless to point the ugliness of such a tracing, of which at least 2 letters encroach upon the lion's mane and tufted tail.

C = the more recent facsimile released by Boucharlat in 1991, fig. 6, and drawn to justify Robin's initial reading: 1/5 longer than A; it still encroaches upon the peripheral ring composed of small ellipses; it is composed of 6 letters and one word divider: ONE LETTER MORE THAN A, with the exception of the 2nd letter from the right, which may easily be recognised in A; the 5 other letters are grossly irregular and entirely different from those of A!!! Finally, the tracing is not made of single lines as in A. It is now composed of double-line designs filled with tiny squares and ellipses on the model of the peripheral ring, which Boucharlat himself had described in 1989 (cf. above, p. 172) as "piquetage," cf. also below, p. 183, the case of the "circulus."

| In Boucharlat's 1989 paper, p. 121: | | In Mouton's 1990 paper, p. 90: | In Boucharlat-Mouton's 1991 paper, p. 33, caption of fig. 6: |
|---|---|---|---|
| Robin: "3rd cent.B.C." | Pirenne: a much earlier date | 3rd-2nd cent. B.C. | "late 3rd-1st cent. B.C." |

Now, how does Robin account for all preceding blunders in Raydān, 6? Robin's

answer is ... a complete silence, viz none of the main details given above is acknowledged by C. Robin (p. 82) who refers to publications some of which I could not get through the normal channel of Parisian booksellers. To write to the authors would amount to the humiliation of stooping under their "Fourches Caudines;" those papers remain UNpublished. Robin also reproduces a poor photo in p. 186 (pl. 42, top) as well as the same facsimile as C of above, p. 173; and the names of both the photographer and the sketcher STILL remain CNRS's secrets! The photo shows how crooked the facsimile really is; f.i. EACH letter is distorted in both form and tracing, viz

1 - the letters are in stippling, viz the dots are very often isolated from each other; for examples of stippling used in names, beard, etc, cf. above, pp. 74-78;
2 - no lateral line is holding the dots in line; and
3 - the initial m does not encroach upon either the peripheral ring and/or the lion tail.

Furthermore, Robin still maintains the understanding of the design as "une scène de chasse" (p. 82). How insane it would be to frontally attack a lion in close combat, even protected by a shield: the mere touch of the lion's paw would send it flying in the hunter's face, probably stunning him to the ground. Nobody would even attack a wild boar in such a manner.

Then, Robin explains that the inscription "invisible sous la corrosion au moment de la découverte, a pu être observée ensuite par radiographie" (p. 82). Such a statement is a vicious pinch of pepper in the reader's eyes. Indeed, the only question at stake is to know when the Boucharlat 1989 facsimile was made, viz before or after the chemical cleaning? Robin remains silent... Of course, "les loups ne se mangent pas entre eux." Since it would be normal to release the news as soon as possible, even before the cleaning, I defy any of those authors to be honest enough to tell the reader the facts ... simply and truthfully.

Finally, Robin claims that "dans Robin 1986, j'avais suggéré de dater cette inscription du III^e siècle avant l'ère chrétienne approximativement. Cette évaluation reposait sur la forme du m, qui présente deux triangles fermés" (p. 82). Robin's reason is a voluntary lie insulting the reader who is taken for an ignorant jackass: the close triangles of m are Pirenne's main characteristic of her A (6 phases)-B (7 phases) groups, A2-B4 being, in her cerebral comput, between 460 - 350 B.C., with her A (4 phases) remaining without any date (cf. her Paléographie ..., Brussel, 1956, p. 137)...and Robin seems to be so fanatically blinded for hoping that the reader might forget that Pirenne had claimed a much earlier date, as indicated above, p. 173. Furthermore, the date ascribed to the locus where the bowl was found establishes the terminus post quem



non, and nothing else.

IV - Ja 3230 = Mleiḥa 1.

The shocking case of the inscribed bronze bowl fragment exposed above is still better than that of Mleiḥa 1 which is ascribed to the same site although the exact location of their origin remains unknown (cf. R. Boucharlat, l.c., p. 120).

The 1st text, Mleiḥa 1 = Ja 3230 was seen in 1970 by J.C. Wilkinson (cf. Water and Tribal Settlement in South-East Arabia, Oxford, 1977, p. 135), note 6) who entrusted his copy of the text to A.F.L. Beeston, and the omniscient Boucharlat states that the original stone disappeared "depuis une dizaine d'années" (p. 120), viz about the time of the publication of Wilkinson's book. How does Boucharlat know that the stone was not removed immediately after Wilkinson's visit? No answer, of course.

Beeston spends a note of 7 1/2 lines to yet another example of the well-known confusing "Beestonian" twaddle he is famous for. He first confides to the reader that he did always have doubts about the South-Arabian language of the Ḥasaean texts without saying that that problem has been solved long time ago. His statement, however, is to introduce his claim that the language of this particular text indeed is South-Arabian. Instead of spelling out his "doubts," which in any event could not be checked and would be irrelevant to the question at hand and without any interest to the reader as well, Beeston would have been on the mark had he pointed out that the introductory expression nfs/wqbr is used in both Ḥasaean (f.i., Ja 1044) and in Sabaean (f.i., CIH 450), and that the type of letters used in the text is the important element in determining the language of the inscription. Since the lettering may be Ḥasaean (but, there is no proof available thanks to Beeston's open refusal to give any factual detail on the text itself and its contents as not typically Sabaean), the present text may be ascribed to Ḥasaean. Furthermore, Beeston adds that the text "seems to be approximately of the fifth century B.C. or perhaps a little later, but not much." This dating could suggest that the lettering is that which is normally used in the Ḥasaean texts. Then, it is impossible to understand how Beeston, who has always been a fervent admirer of Pirenne mini-chronology, could think of such an early date, and his date is too early by at least one century. It is, nonetheless, on the sole basis of Beeston's concoction that Wilkinson (p. 128) claims that "an important new piece of archaeological evidence ... finally proves contact between Old South Arabian ... and northern Oman." The acknowledgement of such importance is pharisaical; otherwise, the original would have been photographed, but Wilkinson does not mention having taken a single picture of the stone. He did, however, make a "transcription" of the text. How thoughful of him! Here again, were his acknowledgement of the historical importance of the text sincere and genuine, he would have published his alledged "transcription" of the inscription. That reproduction would have taken, f.i., about

half of "Table 2" (p. 22) which is entitled "Pearl Fishing in the Gulf in 1907," a subject that is hardly of any importance in "A Study of the Aflāj of Oman" (pp. iii and 1). And, when the reader sees that the whole p. 136 of Wilkinson's book is blank less four lines only when compared with the 52 of p. 135, he realizes how shocking both Wilkinson's and Beeston's attitude really is: there is so much blank space in the book waiting for the publication of Wilkinson's copy of the text that the absence of any facsimile in the book is a peremptory indictment against both Wilkinson and Beeston since they both deliberately rejected an important historical information which is simply discarded to leave all space available for their fancy concoctions.

In an apparent move to show up his well-known pan-classical Arabic fanaticism, Beeston states that inscription reads as follows:

"'gravestone and grave of Dhariyyat servant of the kings'
(nafs wa-qabr dhariyyat fatā l-mulūk)."

Apparently fully satisfied by his own Arabic wording, Beeston was convinced that everything was said and even that the transliteration of the original was superfluous. - Neither Wilkinson nor Beeston care enough to inform the reader whether the text is in one, two or three lines. - Beeston's name "Dhariyyat" could correspond to any of the 4 following names, viz. dryt, dryyt, ḏryt and ḏryyt. Even Wilkinson uses the same confusing rendering; cf., p. 266: "hadhar (ḥaḏar)" (cf. already, p. xiii) with ḏ, and "madhhab" with d (maḏhab). Cf. Arabic darîy "a dog, a young dog [habituated, or accustomed to the chase]" (cf. E.W. Lane, An Arabic-English Lexicon, p. 1789c) and darîy "a shelter" and darîyat "a she-camel by means of which one conceals himself from the object of the chase" (cf. E.W. Lane, l.c., p. 965B). - The word "fatā" mentioned by Beeston is straight classical Arabic فَتًى "a youth or young man;" a strong youth or young man; inexistent in South-Arabian, it corresponds to South-Arabian ġlm "youth" (f.i., Ja 576/2); and "servant" is South-Arabian ʿbd. Furthermore, an expression, such as "servant of the kings," is impossible in South-Arabian because of the generality of the 2nd word. ʾmlkn is a personal name of the well-known ʾqtln nominal derivation; cf. Sabaean ʾlḥyn of Ja 2910/1-2 and ʾbšrn of Ist 7630/5.

I suggest reading Ja 3230 as follows:

nfs/wqbr/dryt/ʿbd/ʾmlkn

Tombstone and grave of Daryat, servant of ʾAmalkân.

In the case of the history of this inscription, Robin is almost tight-lipped, viz

"La première, signalée par Wilkinson ..., a disparu. A.F.L. Beeston, à qui ce document a été soumis, lit et traduit (ibid. et lettre du 29 mai 1986):



nfs[1] w-qbr Dryt fty ʾmlk[n]
Gravestone and grave of Dhariyyat servant of the kings."

Robin's lack of quotation marks makes it certain that his transliteration has applied to Beeston's text his own peculiar orthographic gimmicks.

For fty/ʾmlkn, cf. my remarks above, p. 176. It is highly significant that Robin does not raise any question on "fty" and accepts it as South-Arabian. Of course, whether pan-classical Arabic fanatics or not, whether they belong to two different groups or not, "les loups ne se mangent pas entre eux" (cf. already above, p. 174). Finally, Robin adds that "Il n'est pas impossible d'ailleurs que ʾmlk[n] soit un nom propre." It cannot possibly be anything else; cf. already above, p. 176.

V - Ja 3231 = Mleiḥa 2.

The 2nd of the Mleiḥa texts, Mleiḥa 2 = Ja 3231, was even in a considerably worst condition than the two others.

The only information available was that the original was examined "à nouveau récemment" (cf. Boucharlat, l.c., p. 120) by C. Robin who conceded to release the information that the text deals with "le tombeau et la sépulture" (« What an absurd tautology!! ») " d'un dénommé ʿUbayda" with reference "provisoirement" (cf. fn 26 of the same page) to a 1986 publication a copy of which I have sofar been unable to obtain (cf. already above, pp. 173-174). The name of the person mentioned in the text remains uncertain, viz either ʿbdt or more probably ʿbydt.

Robin released a photo of Mleiḥa 2 = Ja 3231 in pl. 41 (p. 185) of Raydān, 6, where the photographer, whose name is faithfully kept secret by Robin's "béni-oui-oui" club, shows his clumsiness of not even being fit to center the object, both the upper and right edges of the stone are partially cut off; the same awkwardness is also seen, f.i., in pl. 40 (p. 184). Furthermore, the scale remains incomplete: there is no number to indicate its value. It is composed of 10 rectangular (!) 1 cm squares, each one being divided in its longitudinal axis by a single line and printed as a checkerboard.

Robin devotes to this text (pp. 80-81) more space than for Mleiḥa 1 and 2 combined. According to him (p. 80),

> "le texte a été confié pour étude à la Mission conjointe" in Sharjah "que dirige M. Rémy Boucharlat" who, in turn, "a donné [à Robin] toutes les informations relatives au site et aux objets ... et [lui] a confié le soin de publier les textes" (p. 79).

The preceding information had to be reproduced because it raises some reflections, f.i., did Boucharlat try to get from Beeston all the information received from Wilkinson on Mleiḥa 1 = Ja 3230? Robin refers in p. 80 to a letter from Beeston (cf. above, p. 176).

As Robin speaks of the "letter du 29 mai 1986," it has to be the old one. Thus, no attempt was made by either Boucharlat or Robin to get Wilkinson's documentation and, consequently, Robin never saw Mleiḥa 2 and he studied the text from Boucharlat's documentation; in other words, it shows Robin's fundamental nonsensical haughtiness to go ahead with his decipherment and to publish it regardless of his own avowal, viz

"La lecture est rendue malaisée par le mauvais état de la surface; pour de nombreuses lettres notées entre parentheses, la transcription est hypothétique" (p. 60).

The situation unfortunately is much worse than that Robin wants the reader to believe.

-1- His text includes 47 letters + 2 which he claims to be missing at the end of l.4.

-a- As usual, Robin marks the place of a word divider by a blank between two letters. In the present case, however, Robin himself messes up the situation irremediably. The word divider is shown as a blank EXCEPT (1) that at the end of l.1 which is drawn as an aslant stroke, /, and (2) that after his "(d)y" of l.2 which is represented by an hyphen, - . But, his hyphen of l.1 has nothing to do with a word divider as it isolates a noun from the preceding conjunction w and. How blindly stupid can he be to even believe that the reader is a mere robot, a mere rubber stamp as any member of his train!

-b- Of the 47 letters of Robin's reading, 13 are between parentheses, viz each one is "hypothétique" and among them 5 are certain;
6 are erroneous;
1 is replaced by a word divider;
1 remains unread and is marked by a dot; and
1 is an unjustified restoration marked by a dot.

-c- Robin fails to note — I would ... be surprised if he saw it before because of his obsession to find bnt where he thinks it should be - that the initial letter of l.4 can be read s as the short diacritical bar of s is there. The study of the original only would determine whether or not that short bar is a feature of the stone, natural or otherwise.

-d- Robin claims that "les lettres en forme de cercle (w et ʿ) sont de dimension irrégulière (comparer les w des lignes 1 et 2)" (p. 80).
How can Robin be so blind, so irresponsible, so deceitful as to talk of the ʿ's "en forme de cercle"? The 3 of them, just like f, are lozenged, and the 2 of l.4, contrary to that of l.1, are smaller than f; on my enlargement of the published photo, the 3 ʿ's measure 1.7 x 2.5, 1.6 x 1.7 and 1.75 x 1.6 cm, while f measures 1.7 x 2.2 cm. The w's of l.1 and 4 with 1.8 x 1.45 and 1.7 x 1.45 are practically of equal size, but narrower than w of l.2 with 1.8 x 1.7 cm. Finally, Robin's remark originates from a distorted mind upset by an unspecified kind of prurit; f.i., none of the 7 n's, the 6 t's and the 3 y's is similar to any other of its series. Therefore, why would Robin make such a fuss about w's and ʿ's, except of course because of his own lack of objectivity and balance?



-2- As he had done in Mleiḥa 3, Robin here again hides from the reader his two blunders immortalized by his CNRS fellow (cf. above, p. 177).

-a- Robin had erroneously translated nfs/wqbr as "le tombeau et la sépulture." Robin's 3-line commentary does not refer to it ... of course.

-b- The last letter of l.1 is read "(ʾ)." Undoubtedly because of his deep respect for the facts, Robin's 3-line commentary fails to inform the reader that he had read "ʿUbayda," thus normally ʿbydt. The reading of that last letter as ʾ is only explainable, in my opinion, as a vicious attempt to somehow save his reading of "ʿUbayda" by suggesting to the reader that it was a "lapsus calami" or a misprint instead of "ʿUbaydāʾ." And to make things even worse, that letter is clearly n, and definitely neither t nor ʾ. This is, in my opinion, another case of Robin's premeditated dishonesty.

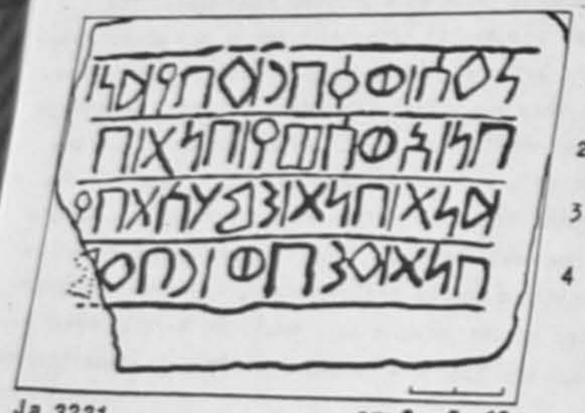
-3- Who in his right mind can understand that, with all facilities at his disposal to travel in Yemen and Oman "aux frais de la princesse," Robin would deliberately chose to publish his greatly limited decipherment based on a grayish photo where, "la transcription [] hypothétique" (according to himself) affects 13 out of the 47 (cf. above, p. 178), thus almost ONE THIRD of the letters, rather to go and secure his reading from the original itself? And if such a trip was either impossible or denied — two hypotheses which appear even at first sight wholly unthinkable — why did Robin not request his fellows of "la Mission française ... à ash-Shāriqa" (p. 80) to send him the squeeze of the inscription? How could that "Mission" refuse such a service to a CNRS's official representative? At last, he would have shown that he had done all that he could to get the best possible documentation on the text. Instead, he proved once more that he prefers his own wild imagination at the expense of a text. So he will never be able even with the help of his sponsors and his "béni-oui-oui" members to erase his present blunders.

Witnessing the irreversible dismantling of A.F.L. Beeston's "laudemus invicem" club and anxious to fill the void with his own version of a rejuvenated "béni-oui-oui" group, Robin carefully built up progressively his own court around his niche of the primary lunar god, leaving empty for the time being - and probably for good because he cannot stand competition, opposition or simple emulation - the niche of the stellar god , while enthroning A. Avanzini in the niche of the solar goddess. Alas, as Pirenne was purposely abandoned and discarded on a Ḥaḍrami pist, seemingly the first visible step to make the change of the guard possible, someone else had also to vanish on a Bayḥānī trail to make room for the new selected solar deity. As a consolation price or some kind of pacifier, W.W. Müller became the "official" newsmonger of the new "béni-oui-oui" group with no new ribbon, no new title, but the old one refurbished with the Medi-

terranean azure framing the logo of the new pair on the blazon.

Meanwhile, the new tenant of the lunar niche inaugurates his new social status by showing his predecessor that his policy will endure. One characteristic of A.F.L. Beeston's literary production is that he learns what pleases him and discards out of hand what displeases him. An outstanding example is his incompetent publication of some Ḥaǧrami graffiti in G.L. Harding, Archaeology in the Aden Protectorates, London, 1944, pp. 51-59 and pl. 52-53, 55 and 56, and my answer in RSO, 40 (1965), pp. 288-299. Although being shown what kind of trashy edition his paper really is, Beeston started all over again with his so-called study of a few graffiti from central Yemen in AAE, 1 (1990), pp. 16-19. S i m i l a r l y, Robin had also failed miserably in publishing a few graffiti from al-Ḫaṣṣā'in in Semitica, 28 (1978), pp. 103-128 and pl. 1-6; cf. my Miscellanées..., I (1979), pp. 34-70 and pl. 6 and 7. He now recidivates in the publication of Mleiḥa 2 on a scale larger than in Semitica,24 (1974).

My tracing, transliteration and translation of Ja 3231 = Mleiḥa 2 are as follows:



Ja 3231 cm 0 5 10

1 nfs/wqbr/ᶜbydn/

2 bn/ʾwsṯy/bnt/b

3 dnt/bnt/šmᶜtktby

4 bnᶜt/ǰᶜšbw/rbᶜᶜm]

1 Funerary slab and tomb of ᶜUbaydân,

2 son of ʾAwsaṯay, daughter of Ba-

3 danat, daughter of Šamatkatbay,

4 daughter of ᶜAšbaw, (one) quarter.

L. 1: ᶜbydn, same ǧtyln derivation, f.i., as Sabaean ǧtyln of Ja 560/7, and Qatabanian dʾyb(n) of Ja 196. In my Pièces épigraphiques de Ḫeid bin ᶜAqîl, la nécropole de Timnaᶜ (Ḥaǧr Kohlân), Louvain, 1952, p. 83, the name should be read dʾybᶜnᶜ; as shown by my squeeze, 1.3 cm of the lower half of n is still on the stone; thus m or t are excluded; the lower half of ʾ also is 1.3 cm high. Cf. ᶜbydm in Qatabanian (f.i., Ja 79/1) and Sabaean, Ist 7628/1.



L. 2: ʾwsṯy, same ʾqtly derivation, f.i., as Sabaean ʾkryy of RÉS 3946/5. Cf. also wsṯ, masculine name in Safaitic (f.i., Field 1039) and Thamudic (f.i., HaLi 461) and feminine in Safaitic Wi 503.

LL. 2-3: bdnt, cf. Sabaean bdn of Ja 2574 k, and bdnn of Safaitic Cx 102.

L. 4: for the initial letter of the line, cf. above, p. 178. ᶜšbw, same derivation f.i., Sabaean rbḫw of RÉS 4608, tqlw of Ja 2774 bw, and especially the Minaean feminine name ḥtmw of Gl 1250 (= 970)/2. Cf Minaean ᶜšb of Ja 2250 b, and Safaitic ᶜšbt in Safaitic WH 2245 and Liḥyanite JaL 50 g. The initial name of WH 2408 reads ᶜšrn, as in G. L. Harding, An Index ..., p. 422, instead of "ᶜšbn" of F.V. Winnett and G.L. Harding, Inscriptions from Fifty Safaitic Cairns, Toronto, 1978, pp. 361 and 595A. - rbᶜ, cf. Arabic rubᶜ "a fourth part."

The 3 important pieces of information bulldozed into nothingness by Robin's wild imagination are such that they make this text of exceptional value and unique. They may be presented as follows.

-1- The text gives the matrilinear ancestors of the author up to the fourth generation, viz the great-great-grandmother, thus one generation more than the Ḫasaean text from Thâj published in Atlal, 6 (1402/1982), pp. 139-140, photo in pl. 124A and Arabic text in pp. 136-137. For a lot of speculations and cerebral wanderings around this Atlal text, cf. J. Ryckmans in S.H.A. al-Khalifa and M. Rice, Bahrain Through the Ages. The Archaeology, London, 1986, pp. 407-417; on p. 407, correct the translation "Ǧdyt" to "Ḳdyt" and "ᶜhdt" to "ʾḥdt."

-2- Most important is the fact that the author is not a woman, but a man called ᶜbydn.

-3- The tomb was divided into four sections, one of which is occupied by the male author.

What a pity to see such exceptional features drowned in the murkey waters of Robin's wild imagination and pan-classical Arabic fanaticism!

Complementary note: C. Robin on Ja 1048 and 2883.

A - Ja 1048.

I have to apologize for the text on ᶜḥns (ll. 2-3) of my Sabaean and Ḥasaean Inscriptions from Saudi Arabia (Rome,1966), pp. 74-75: ʾḥns instead of ᶜḥns. p. 106A: yḥns

I do not understand what happened, and although my copy was corrected in 1967, I failed to publish a note about it in my Miscellanées.

Already more than 20 years ago in Semitica, 24 (1974), pp. 83-125 and pl. 1, Robin released an article full of illogical, "latius hos" reasonings and all kinds of errors that I became used to call "Robineries. " A short example of Robins' vicious method is that of p. 113, repeated and explained in p. 114 ,where he speaks of

"le m [of his "wd²bn" of Ja 1054] est partiellement visible sur la photographie tronquée" (p. 114).

There is no m, but as my edition mentions, there is a mere word divider because there is no trace of the right extremity of the lower aslant bar that would belong to m. And Robin cannot even write his native language correctly: his expression "la photographie tronquée" is a lie: the photo is complete; both sides of the text were cut off (= "tronqués") by the camera (cf. also below, p. 184). It is in that paper that Robin mentions bnt daughter in "Ja 1048/2-3 (que je lis bn[t]/Sbt/bnt...)" (p. 113). This reading is another proof of Robin's vicious dishonesty, to which I did not answer at that time because of the hopeless case of its author. Unfortunately, J. Ryckmans (cf. l.c., p. 414, note 13) writes "if one accepts the reading mtmt/bn[t] sbt bnt ʿhns d²t ... proposée par Robin, loc. cit., 113," where not only "ʿhns d²t" must be corrected to ʿḫns d²t, but also Ryckmans' reference to "113" is wrong for ʿḫns; it is in p. 116 that Robin lists Ja 1048 in fn 1 which deals with texts containing ḫ. Note also Ryckmans' natural, devious evaluation of Robin's opinion. The all-flatterer Ryckmans does not want to hurt his former disciple; he prefers to swim in troubled waters.

The following remarks are in order.

(1) The voice of haughtiness is manifested by "je lis."

(2) Although having no proof for restoring t after bn in l.1, Robin needed a t because of his simplistic belief that mtmt ending with -t had to be feminine!? The white speck in the form of a large L supports 2 letters that are perfectly clear, viz f of nfs (l.1) and n of bn (l.2). Therefore, had there been a t after n, it too should be plainly visible ...., yet, there is not a trace of it. But, Robin "s'en fout copieusement." What counts for him is not at all what the engraver wrote, but what his wild imagination trained at the school of both M. Rodinson and J. Ryckmans, has dreamed to prove.

(3) Robin reduces the monogram of l.2 to a single letter without even mentioning or justifying his scalping. Of course, CNRS's Robin does not have to justify anything.

(4) The upper part of ² (l.2) is clear on the stone.

(5) The upper triangle of l (l.2) also is clear on the stone.

(6) There is no bnt in l.3. Had Robin written "1048/2-3" instead of "1048/2 and 3" the reader would have understood that he meant the genealogy itself, and it would have been correct. But, he did not.

(7) The case of ʿḫns brings up the matter of onomastics. Robin deals with his "onomastique" in p. 114, but he fails to mention ʿḫns. Obviously, it is an important onomastical feature of the text; but such a nominal derivation obviously is wholly foreign to classical Arabic. For the ʿ[= ʿttr]+qtl derivation, cf. the Sabaean names ʿlhm of Ga 20/8 (a place) and Ja 2893/1 (a man); cf. also the ʿqtlm derivation in Sabaean ʿgšbn (a man) of Ja 2960 ac.



(8) Blinded by his pan-classical Arabic fanaticism on the genealogical element bn- in South-Arabian Arabic onomastics, Robin claims the "apocope du ²" to be "possible dans le n. pr. Ydʿb (RES 4685/5-6)." Robin lists that point as one of "les quelques traits de language" although it is a point of onomastics. This confusion of words in Robin's style is not surprising at all; here is another one. Since he deals with ²b > b, it would indeed be an "aphérèse," the opposite of an "apocope." That kind of aphaeresis is unknown and unacceptable since the root dʿb does exist and is even attested in the personal names dʿb in Liḥyanite JaL 102 e and Thamudic dʿbt in HaLi 17 A. Another example of Robin's (and also Boucharlat's) confusion of French words is their endorsement of a facimile full of "tiny squares and ellipses" as a "piquetage" in Mleiḥa 3 (cf. above, p. 173); cf. also below, this page, the case of "circulus."

B - Ja 2883.

I wish to end this section on Robin's blunders which unfortunately are frequent as well as plainly loathsome on Ja 2883. It takes six pages (pp. 102-107; viz 253 lines) to build up his case. What a waste of paper ... and of time for the reader to go through all that scenario!

— 1 — His first salvo deals with RÉS 2687.

a - Robin states that "Rhodokanakis [] a lu 'Yashkur²il' parce que ce nom est attesté ailleurs."

This statement contains 3 qualified errors:

(1) Rhodokanakis reads "IŠKR²L" with no vocalization at all;

(2) Rhodokanakis does not give his reason;

(3) At any rate, Robin is so loathsomely blinded by his own wild imagination that he DOES EXACTLY that with which he reproaches Rhodokanakis, viz he made up his mind to read "yšhr²l" "parce que ce nom est attesté ailleurs." How one-track minded can you be? What could be more appropriate than the centuries old "Medice, cura teipsum?"

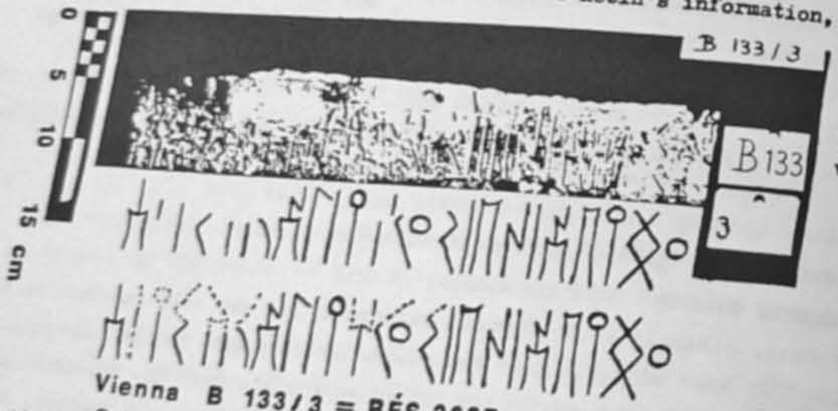
b - He states that "seul le r est sûr; quatre lettres sont surmontées d'un circulus qui les signale comme douteuses, le y, le s², le ² et le l; enfin, du k éventuel, il ne reste pas trace (Rhodokanakis 1927, p.69,n.6)."

Here again, Robin gives his reader another proof of his need to go back to school because it is stupidly snobbish to call a simple dot a "circulus" (cf. above, this page for another example of confusing the words). As his reference proves it, Robin summarizes Rhodokanakis who uses dots. Such confusion seems normal for Robin; cf. already

above, p. 173 C, where the dots of the photo became squares and ellipses!? Finally, Robin's note on the letter k is also a lie because Rhodokanakis puts it between two brackets and does not say that there is no trace of it. He simply could not read it; this is different; cf. Altsabäische Texte, Vienna, I (1927), p. 69, fn 6.

c - Robin then refers to Doe's photo, his (?) visit to al-Binâʾ and his (?) photo of pl. 48, no. 1, which is much less legible than Doe's because, as usual, it is grayish. What a waste of paper caused by the fallacious claim to publish a photo, regardless whether it is legible or not!

d - Much worse, Robin recidivates here his failure on squeezes, as in Ja 3231 = Mleiḥa 2 (cf. above, p. 179): he made no attempt either to see or to get photos of the 6 squeezes, nos. 1-5a, of the Ohne text = RÉS 2687 in the Vienne series B, viz B 133 (cf. M. Höfner, Die Sammlung Eduard Glaser, Vienne, 1944, p.89): those squeezes represent the physical situation of last century. Obviously enough, Robin could not care less about them so that he may leisurely follow the dictate of his wild imagination. The entire name in question is in Vienna B 133, 3rd sheet, upper left corner. Here follow an enlargement of that line and my two facsimiles. For Robin's information, this is an example of a "photographie tronquée" (cf. above, p.182).




Vienna B 133/3 = RÉS 2687

The six letters "[illegible]" are penciled above the letters on the squeeze itself; the last four can still be distinguished on the reproduced photo. Contrary to Robin's claptrap, his restoration of "[h]" after yš is contradicted by the remains of the two (not one only) vertical strokes between š and r. Robin will undoubtedly maintain his erroneous reading in the same manner that J. Ryckmans still maintains his false opinion of "n" = 1st singular personal pronoun in some Sabaean names, a thesis that the bulldozer Robin endorses (p. 14) - naturally - as if it was a certainty without even being honest enough to register my rebuttal.



— 2 — Ja 2883 •

Robin's 2nd salvo is even more vitriolic than the 1st, and deserves to be reproduced in extenso as an example of his demented wild imagination.

> " Bien qu'Albert Jamme présente sa lecture comme ... 1
> certaine, un examen de la photographie montre qu'il n'en est rien : on déchiffre ... 2
> $Ys^2..'l$, peut-être $Ys^2(h).'l$ (pour la forme du *h*, qui évoque une fourche, se reporter à *mḥfd*$^{hn}$, dans le même texte, à la ligne précédente). La quatrième lettre est ... 3
> difficile à reconnaître : il n'est pas sûr que ce soit un *r* ou un *ḥ*. Il est manifeste que ... 4
> la lecture d'Albert Jamme se fonde davantage sur les noms attestés que sur les vestiges de lettres et il n'est nullement impossible de restituer $Ys^2(h)[r]'l$ " (p. 104).

Here again, and for the 3rd time in this short paper (cf. already pp. 179 and 194, respectively for Ja 3231 and RÉS 2687), Robin fully satisfied by his wild imagination puts his blind trust in a photo, exclusively of the most reliable information after the original, a squeeze made shortly after the discovery. Here again, Robin could not care less provided his wild imagination and his senseless lust are satisfied. All my following remarks are based on my two latex squeezes of M 489.

Ad 1 - This mere affirmation proves only that the bone that was pestering Robin's throat was sharp; it was thus better to spit it out than to choke on it.

Ad 2 - Robin states that the 3rd letter is "peut-être ..., (h) ... (pour la forme du h, ... ligne precedente."

Robin's dishonesty already appears at the start. The h closest to the royal name is the preposition h introducing mrʾsmn (l. 5, end) which precedes immediately the king's name. The letter is a rectangle with an open top, thus with two right angles only. The h of the article -hn of mḥfdhn (l. 5, center) is also a topless rectangle. Blindly hypnothised by his obsession of "yšhrʾl," Robin mistook a natural circular shell in the stone, the diameter of which, 1,7 cm, is even somewhat larger than the half of the letter height, 3.3 cm; and the width of the periphery of that circle is almost thrice as large as the tracing of y and š. The 3rd letter is r; h is manifestly impossible because there is no vertical stroke there.

Ad 3 - The 4th letter ḥ is composed, as that of both mḥfd (l. 5) and ḥḍrmt (l. 6), of a long stem of 2.6 cm supporting a trident with two right angles, wide of 1.6 cm. These measurements are also those of ll. 5 and 6.

Ad 4 - As far as I am concerned, Robin is free to say and print whatever he wishes, including lies of which he does not deprive himself; f.i., Robin states that "sans doute se fonde-t-il [= R.L. Cleveland] sur un avis d'Albert Jamme" (p. 105). This is another qualified lie. All those untrue statements and/or inuendos may lead to the conclusion that Robin has become a professional of that kind of activity.

VI - Conclusion.

The preceding inscriptions exemplify the piteous mistreatment imposed upon epigraphical material by some individuals who do not hesitate to bend the basic requirements of publication to suit the whims of their self-conceit. Both Beeston and Robin would doubtlessly argue that they are not publishing Mleiha 3 but simply bringing it to the scholars' attention. Nobody would be gulled by that semantic hairsplitting which would exonerate them from misleading scholars and from omitting the basic information, viz the simple copy of the text or at least its complete translitération, viz the fundamental requirement, which would have been expected and, therefore, most welcomed, instead of their own concoctions which are duly rejected. Furthermore, the attitude of Bouchariat - Mouton tandem in the case of Mleiha 1 = Ja 3230 is so serious as to annihilate their credibility. Robin, too, is guilty of grave negligence in not ascertaining the correctness of the first facsimile. He apparently did not trouble himself to check the facsimile. The preceding experience is quite sufficient on the contents of either an antiquity or an inscription, their testimony will not be accepted unless supported by a three above-listed writers may in the future claim in their presentation of either an clear, readable - not grayish as "à la Robin" - photo accompanying their statement. This remark reminds the reader of the aberrant basis of C. Robin's epigraphical research formulated in his 1977 thesis, viz "une publication d'inscription sans photographie est inutile" (cf. Le pays de Ḥaulān et Ḥawlān Qudā'a (Nord-Yemen) avant l'Islam, Paris, p. 390, ll. 7-8). Unfortunately, my Miscellanées d'ancient arabe (Washington, XIV [1985], p. 279, l. 3 from bottom) was able to state that "Robin is manifestly dishonest because he does exactly what he condemns" (cf. above, p. 127). In his 1992 book entitled Inventaire des inscriptions sudarabiques, vol. I (published by the Académie des Inscriptions et Belles-Lettres), he cynically recidivates on a huge scale by dealing with 38 unphotographed texts out of a group of 130 inscriptions (thus, almost halfway between 1/3 and 1/4) and 28 of them are among the longest and most important documents of the whole group. My review of his 1992 publication, finished in 1993, will be published later. As shown above in the case of N. Rhodokanakis (p. 183), Robin again "does exactly what he condemns."

Mleiha 2 = Ja 3231 (p. 179) is the 3rd example after Ja 2883 and RÉS 2667 (pp. 185 and 184, respectively), which illustrates Robin's devious working method of not heeding the exceptional importance of squeezes and especially those made long time ago because they preserve a condition which may have been more or less altered by man and/or climateric conditions. A painstaking, meticulous study of the originals is not Robin's speciality, and the consultation of old squeezes does not belong to his daily schedule of research no more than it was in the daily work of both M. Rodinson and J.



Avanzini; in other words, his wild imagination is in constant action and grayish photos are his best allies.

The end of my review of Robin's publications of some al-Ḫaṭ'in graffiti in my Miscellanées ..., X ( November 7, 1979 ; cf. above, p. 180) contains the two following excerpts, viz

"In my last three publications, special attention was devoted to Ch. Robin's writings because even a quick glance taken at them revealed an ominous reality, and their study, alas, confirmed the original impression and brought to light such an assorted collection of ugly irregularities of all kind, viz. inconsistencies, errors of judgment, voluntary and unvoluntary blindness, slanders, distortion of the material, partisanship, etc., that the reader can hardly believe his own eyes" (p. 65) and

"SA science, therefore, has to further brace itself against that second generation's onslaught which is only in its beginning" (p. 66).

This general appreciation, although written more than 15 years ago, is unfortunately confirmed by my preceding review of Ḥaydān, 6.

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Additional note to be added to p. 81C.

When tidying W.W. Müller's idiocy (pp. 81-89A), I had no idea that Ḥaydān, 6, although expected, would arrive soon. One contribution of C. Robin is on the Mleiha texts studied in the last section of my volume. Here again, the publication of new material demanded the rewriting of that section of my volume. It was done without hesitation as soon as Müller's case was finished.